

LETTER FROM THE PRESIDENT RETOOLING IN COMPARATIVE RESEARCH

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Most of us, members of the Comparative Politics Organized Section, established our bona fides as professionals through some form of field investigation, in which we became experts (or at least "one eyed kings in the land of the blind") in a particular segment of international real estate. Many of us received our initial appointments filling slots in departments that lacked an expert to teach courses and guide graduate research on the "government and politics" of the region or continent through which we slogged in preparation for writing our dissertations.

The model of a political science department with at least one expert for every region has many advantages. For one, typical American undergraduate students tend to be both intelligent and ignorant. Consequently, courses that provide thick descriptions and informed interpretations of politics in foreign countries fill a crucial need in our curriculum.

Second, advanced graduate students who themselves are planning field research for their dissertations need the guidance of faculty members who know the regions intimately. Graduate advisors should know about recently completed research and research already under way in their region, such that their graduate student's proposed dissertation does not rediscover the Mediterranean. Advisors should also know where data can be best collected, who the local experts are who can be of help, and who the funding agencies are that provide stipends for research in that area of the world.

Third, colleagues in one's department will want an ambassador, as it were, to the

area studies conventions for each of the world's regions. Debates and discourses in one region (for example, dependency theory in Latin American studies in the 1970s; or corporatist theory in European studies of the same era) tend to migrate into work in other regions. Therefore, all political scientists need to have colleagues who can represent discourses of "their" regions of expertise to colleagues who ask for help in getting sources and insights from those discourses.

Fourth, political science departments can justify more faculty appointments to the extent that they can convince deans that all political science departments must have an expert for every region. Thus, the idea of an expert for every region is a job enhancing model for our profession.

Finally, the wider university community profits immensely when there is local expertise on contemporary issues in an area of the world that suddenly becomes headline news. Political scientists are useful resources for campus panels and local me-

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dia when their regions of expertise become areas of political or economic crisis. Given these points, it would be madness to advocate disbanding a model that serves so many constituencies so well.

The model of a political science department with an expert covering every region, although quite attractive, presents an extremely difficult dilemma for young scholars seeking to define and develop a second research project after the publication of the results of their dissertations. They have invested heavily both in general regional expertise and theoretical knowledge of a particular problem (democracy, economic development, equality, etc.). They can then address a theoretical issue best analyzed within the confines of that region. Generally, a comparativist dissertation points to a variable, or an aspect of the local context, or a historical conjuncture, that motivated some important outcome in unexpected (from the point of view of existing theory) ways. Our theory is enriched, or perhaps undermined, by excellent comparativist dissertations that are built on field experiences.

Once the dissertation project is completed, the comparativist who seeks to continue doing research can follow one of two paths. From the point of view of a vibrant research program, the logical path would be to find a different place to examine the theoretical issue that motivated the dissertation research. In a different setting, the comparativist can vary the context, or set new parameter conditions, or better isolate the variable that in the dissertation was found to be of greater significance than previously recognized. This, however, is costly, because it would involve the development of new regional expertise, perhaps a new language, and maybe even the trespass onto areas "covered" by a departmental colleague, leading to redundancy in one area, and a loss of coverage in another.

The second path, that of examining a different theoretical issue in one's region of expertise, is therefore most often taken. Through this strategy, a comparativist can reap returns on area expertise, and can continue playing an important role for undergraduates, graduate students, colleagues and the wider community as the local expert in

a particular region. Moreover, the dynamic quality of politics is helpful to us in this regard. As Africanists, we can be experts on nation building in the 1960s, debt in the 1970s, democratization in the 1980s, and state decay in the 1990s. Latin Americanists can alternate being experts on democratization and bureaucratic authoritarianism depending on the decade. Experts on the Soviet Union, who invested in Russian expertise, now have a whole menu of issues that can be analyzed.

While this second path appears to be the one of least resistance, I believe that we have collectively underestimated the costs of retooling for a new theoretical issue, and overestimated the costs of retooling for expertise in a new region. I have seen this problem with particular poignancy reading articles on African debt problems in the 1970s from distinguished scholars who never studied economic theory; and I see it today from students of the former Soviet Union studying nationalism, who have not immersed themselves in the 1960s debates, mostly among Africanists, over primordialism, situational ethnicity, and the sources of ethnic violence. Changing theoretical focus is expensive, if we are not to replicate the mistakes of scholars who worked years earlier on the problem that is now being addressed.

There are two implications of this course of reasoning for our professional lives. First, our departments and universities must understand that a second major research project for all comparativists requires substantial re-tooling, perhaps more so than in other sub-fields of political science. If our research careers are to develop, we must therefore be given the time and resources necessary to do so. This is the case even if our second research project compels us to walk on the same real estate over which we trekked in the course of our dissertation research.

A second implication is that the logic of theoretical development may lead scholars down the first path, of needing to develop area skills in a new region. We must build into our hiring and promoting practices the opportunity, or even the encouragement, for taking such a path. To do this,

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**Latin (cont.
from page 1)**

NEWS, NOTES, AND COMMENTARY

THE RETURN OF THE SON OF THE BRIDE OF THE FUTURE OF COMPARATIVE POLITICS

In the Summer, 1993 issue of *APSA-CP*, former Section President, Ronald Rogowski, and then incoming Section President, David Laitin, opined on the current state of comparative politics, and mused about alternative futures for the field. In the year that followed, many of the Section's members have expressed considerable interest in the debate, and several have weighed in with their own opinions. What follows are brief excerpts of the previously published editorials followed by two new ones from Ian Lustick (University of Pennsylvania) and Joel Wolfe (University of Cincinnati). The editors of *APSA-CP* are glad to be the vehicle for the continuance of this important exchange which impacts upon all of our careers; anyone interested in throwing in his or her own two cents may do so by writing to Adam Levine, Assistant Editor, at the address on the back cover, or by e-mailing to levine@polisci.sscnet.ucla.edu.

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A specter is haunting comparative politics; it is the specter of pure theory. In two sister disciplines, linguistics and economics, comparativists disappeared once pure theorists generated an intellectual revolution. Comparative linguistics was a vibrant, descriptive sub-discipline (and remains so in Europe) before the Chomsky revolution. Descriptive linguistics at universities in the United States may now be found only at the margins of anthropology departments. By the same token, in the wake of the neoclassical revolution in economics, specialists in the economic systems of different regions or states are no longer hired by major economics departments. The systematic study of the particular dynamics of various economies is now relegated to the political science curriculum.

Rational choice theory, with its claims to axiomatic reasoning, deduction as a fountainhead of truth, and universal principles

of politics, is challenging political science in ways quite comparable to the gauntlets thrown down in linguistics and economics. Having a specialist for every piece of international real estate may soon seem as arcane as having a specialist for every planet in an astronomy department.

I, however, prefer a different course: to engage our data with rational choice theory in order to raise an ever expanding set of questions for political scientists to answer. We should engage more directly with this work, continually tantalizing theorists with uncomfortable data. To use our area knowledge to discover interesting anomalies, to provide plausible deductive accounts of our statistically validated patterns, or to identify theoretically appropriate data sets would be to integrate theory and empirical data far better than has been done in linguistics or economics thus far. This is our disciplinary challenge; it is one that members of this Section should attend to.

Looking at other disciplines, at least three models seem possible. In *economics*, geographical expertise has yielded almost wholly to unified theory. Whatever one needs to learn about a particular economy, most economists believe, can be acquired in the course of research, or through local collaborators. In *physics*, experimentalists long ago were segregated from (but have remained complementary to) theorists. In *history and literature*, regional and period expertise remain trumps; "theory" is but a lens (skeptics will say: a jargon) through which one views or describes.

Adam Smith's fundamental insight about the virtues of specialization inclines me toward the second, or "physics," model. Yet it is worth pondering exactly what such a model means for "engaging our data with theory" in Laitin's sense.

As our theories and models (often derived from economics) grow technically more sophisticated, they demand more training to do, or even to understand well. Scholars will be able to work theoretically only if they do so in a concentrated fashion; and, among this group, regional, cultural, and historical expertise will increasingly be neglected. At the same time rewards and prestige will accrue to those who subject the brilliant theories to the hard tests of empirical accuracy; but (the physics model suggests), good testing will require *both* detailed knowledge *and* a high degree of technical expertise.

The second point, I think, is crucial. Comparative experimentalists must possess: (1) a good enough grasp of the formal models to understand, apply, and criticize intelligently what the theorists propose; and (2) the methodological skills to make their tests convincing.

In an ideal world, then (or, to be more modest, an ideal Department), graduate students will be educated along two distinct tracks: one theoretical, the other empirical in the sense just outlined. Undergraduate teaching will likely involve three distinct levels: "core" introductory courses that focus on models and theoretical issues; intermediate ones that inculcate area expertise; and advanced offerings devoted to systematic comparison.

Chalmers Johnson and others argue convincingly that economics has paid a serious price for ignoring the comparative study of economic systems. At the minimum, economics departments want to find people who can teach a course on the Japanese economy, but cannot. At the maximum, economists have so long ignored data and satisfied themselves with internally consistent models that their models have lost empirical validity. My impression is that, while impressive in many respects, economics explains very little about how economies actually operate.

The physics model is also potentially misleading. One of our main problems in the empirical study of politics is our inability to perform relevant controlled laboratory experiments. We must deal with the real world and data provided by actual events. The 'hard' sciences do not do very well in predicting the weather or the next earthquake because they cannot control for all the factors involved.

The second thing to emphasize is that physicists never assume things that can be shown to be false. In fact, experimentalists work their hardest testing assumptions of new theories. Rational choice in the social sciences clearly fails this test. No psychologist takes the idea of rationality, in the sense implied by rational choice models, seriously.

I know only a little about the Chomsky revolution in linguistics, but what I do know suggests that Chomsky demonstrated that there is a large set of characteristics held in common by all languages. I would argue that comparativists often delight so much in contrasting what is different that we forget to point out the universals. More importantly, in linguistics, after all the universals have been established, there will still be a lot of variance to be explained. In linguistics the residual variance may amount to trivia, things that are best left unexplained, and simply memorized if one must actually speak the language. I find it hard to imagine that the same will prove true of politics. Even after explaining how all political systems work in general, we will still need to understand the workings of particular political systems.

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Area studies is sometimes caricatured as the atheoretical, configurational study of particular areas of the world, cut off from global perspectives, and from theory and methodology. Yet throughout the interviews carried out in this study (see *Rebirth or Stagnation? European Studies After 1989*, Washington, D.C.: Social Science Research Council, 1993), respondents stressed the need to carry out complementary comparative and theoretically-driven problem-oriented research, and to train students in theory and methodology as well as substance.

These are tall orders, but Europe has some unique advantages as a focus of research and training. Stimulated by the recent changes in Europe, by the availability in Europe of a large number of European studies centers oriented towards cooperation with American scholars, and by the activities of supra-national and trans-national research organizations such as the European Science Foundation and the OECD, there is an historic opportunity to make Europe a laboratory for social scientific research unmatched since the expansion of area studies in the Third World in the 1950s and 1960s.

However, effective social science research is more than the transmission and diffusion of method and theory; it also relies on the accumulation of empirical information, its transformation into comparable analytic form, and the contextualization of findings into usable country and area-specific form. As a result, area studies, methodological training, and theoretical perspectives must be combined in a more integrated way that has been true in the past.

This combination of diffusion of methods and theories, accumulation of country-specific data, comparison and integration of findings, and contextualization in different national contexts requires the combined resources of social science theory and methodology — which are best provided in social science departments and institutes — and area studies knowledge, language, and insights — best nurtured and developed by scholars with a commitment to area studies. The problem is not to choose between disciplinary research and area studies but to provide for both side of the equation.

As theory rises in prominence in the study of comparative politics, an over-reaction that rejects traditional methods will surely result in a comparative politics more impoverished than one in which theoretical approaches and traditional methodologies are more evenly integrated.

We argue that American politics, particularly the study of Congress, constitutes a superior exemplar for comparative politics. Four observations about the Congress field are relevant. The first is that theorists have not driven out the close observers of Congress. Instead, cooperation between theorist and empiricist has arisen. Second, theorists' tools are simply inadequate to make detailed, ex ante predictions about the types of behavior that should be observed in any particular legislative context. That knowledge can be derived only from direct observation and detailed empirical analysis. The other side of the coin is our third point. Those scholars who provide the thick description of congressional behavior do not provide complete explanations for their observations. Our final observation from congressional scholarship is that, by and large, theoretical and empirical exercises are seen by their respective practitioners as complementary.

How might such an integration proceed in comparative politics? First, as both Laitin and Rogowski note, a number of such scholars have emerged already. Second, positive theorists have increasingly become interested in culture over the past decade. One of the deep and all-encompassing problems of rational choice models is the multiplicity of equilibria (i.e., the Folk Theorem). This implies that theorists cannot predict cultural patterns in advance; particular knowledge of a given society is absolutely essential.

Third, the methodological divide between positive theorists and many empirical scholars in comparative politics is more than just a contest between abstract logic and in-depth knowledge. It also typically entails the divisions that separate positivism from interpretation. More often than not these are seen as irreconcilable approaches. Recent work, however, demonstrates the compatibility of thickly descriptive accounts and intentionalist accounts.

RATIONAL CHOICE AS A HEGEMONIC PROJECT, AND THE CAPTURE OF COMPARATIVE POLITICS

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In his "Letter from the Incoming President" published in the Summer, 1993 issue of this newsletter (*AFSA-CP*, 4:1), David Laitin cited the damage done to the study of Economics and Linguistics in the wake of their capture by devotees of deductivist approaches to what he calls "pure theory." According to Laitin, the capture of these fields resulted in the displacement of illuminating empirical work by elegant but untestable formal models. Political Science, Laitin suggested, and in particular comparative politics, is confronted by a similar threat — the "specter of pure theory" — a menace that in Political Science has assumed the form of rational choice theory.

I agree with Laitin that rational choice theory is the primary source of this threat in Political Science. I would specify, however, the challenge as posed, not by all rational choice practitioners, but by those who rely on formal modeling and the elaboration, à la neo-classical economics, of increasingly refined corollaries to solve anomalies generated by their own thought experiments. More importantly I disagree with Laitin's (perhaps unintended) identification of rational choice theory (or, as he calls it, "pure theory") with "theory," and his use of the term "theorist" to mean deductivist formal modellers, and only them. This identification leads Laitin to suggest that all theoretically ambitious comparativists within Political Science must acquiesce in the particular definitions of appropriate problems, valid methods, and "good science," advanced by the practitioners of deductivist rational choice theory.

At the end of his brief but provocative essay, Laitin makes an observation that both alludes to formalist rational choice for what it is — a hegemonic project to establish one particular theoretic taste as generic "theory" — while conceding victory to that project well before it has been won. "For comparative politics to flourish in an intellectual world where deductive models are hegemonic," Laitin concludes, "we must both enrich and disturb these models with a comparativist's eye for difference and detail." Thus does he advise comparativists and area specialists to accept a theoret-

ically and conceptually subservient role within the profession — tantalizing the "theorists" with the challenge of capturing the world's complexities in formal models while reminding them, the "theorists," of how blunt their instruments still are.

From my perspective, as a theoretically ambitious comparativist, Laitin gives away the game well before the end is even in sight. Whenever we consider the dynamics of how that which is to be accepted as paradigmatic truth or background knowledge is established as such, Gramsci comes to mind. In Gramscian terms, the deductivist rational choice practitioners are doing nothing more, but also nothing less, than waging a war of position within the field of Political Science, seeking to establish their research program as hegemonic within the discipline. Victory in this war would be signaled if deductivist rational choice practitioners achieved the same unchallenged control over standards for publication, promotion, tenure, and the awarding of research grants in Political Science as neo-classical economists have in Economics. Laitin's equation of "theory" with rational choice deductivism, and his description of deductive models as "hegemonic" in our "intellectual world" represent a premature surrender in this struggle — an unwarranted and damaging concession by an outstanding political scientist whose own work has shown how many productive meanings there can be to "theory," and how impor-

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THE NEED FOR A PLURALISTIC COMPARATIVE POLITICS

The recent discussion about the future direction of comparative politics in the *AFSA-CP* (4:2 and 5:1) focuses on relations between area expertise and comparative theory. In the process, the authors offer different academic disciplines like physics, linguistics, economics, history, and literature as possible futures. Ronald Rogowski sees theories becoming more technically sophisticated, more difficult to employ and to understand. David Laitin projects public choice theory as capable in unifying the field, yet needing to be balanced by the details of area expertise. Kenneth Shepsle and Barry Weingast argue that formal theory and data can be combined, citing US Congressional scholarship. Sidney Tarrow, reflecting on the effects of recent changes in Europe, emphasizes the new imperative for integrating area studies capacities, methodological skills, and theory.

These scholars all seem to imply that comparative politics should move toward a more pragmatic science, where theoretical consensus facilitates the use of sophisticated methodologies and subtle empirical complexities. In contrast, I argue that comparative politics is best served by maintaining and developing its current pluralism.

First, the image of comparative politics as more formal, technical, and empirical fails to acknowledge the diverse topics and methodologies existing among comparativists. Different ways of defining the field reveal a multiplicity of competing ideas about what comparative politics is and how it should be done.

One common criterion for defining comparative politics is substantive. The subfield studies politics through comparison of political systems, in part or in whole. In practice, this often leads to focusing on one country or region of the world. Other, if fewer scholars, examine institutions or policies cross-nationally. Most would accept subject-matter as one, if not the only, distinctive aspect of the field. This leads to a wide range of topics and realms of experience.

A second way of defining the subfield is in terms of its methodological technique, the comparative method. Arend Lijphart and others like David Collier have developed this line of thinking. In this view, the comparative method contrasts with and is

equivalent to the experimental, statistical, and case-study methods. Yet, area and regional specialists know the validity, importance, and incommensurability of interpretive understandings.

A third approach points to the diversity of theoretical schools characterizing the field. Roy Macridis and Bernard Brown identify a clustering of schools in terms of topics and concepts as characteristic of comparative politics, while Howard Wiarda surveys the rise of development theory and its fragmentation into competing frameworks of analysis. In terms of theoretical methods, comparative politics is rich in alternatives.

Second, the diversity of topics and methods in comparative politics means that it is open to wider and more diverse array of scientific practices than "paradigmatic" disciplines like physics, biology, or linguistics. When comparative politics moved beyond the legal institutionalist approach of the late 1940s, positivism became the dominating philosophical foundation for behavioralism, functionalism, and systems analysis. By the 1970s, positivism was being superseded by the rise of philosophical realism, seen in the emergence of the new institutionalism and public choice alongside and in competition with the older positivistic approaches. Other perspectives from post-modernist and pragmatic thought are now joining the fray.

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MR. KRUGMAN GOES TO WASHINGTON

In a dizzying spate of recent articles and books, MIT economist Paul Krugman criticizes the Clinton administration's "wrong and dangerous obsession" with international competition in the export of high-technology goods. Krugman's objection to this now-fashionable portrayal of trade in terms of international rivalry is that it represents a repackaging (or, in its zero-sum emphasis, a perversion) of many of the essential insights of the "new" economics of trade theory that he and others pioneered beginning in the late 1970s. Policy "entrepreneurs" supportive of managed trade and activist industrial policies, Krugman argues, seized upon strategic trade theory as confirmation that other nations used strategic policies to enhance their citizen's living standards, and to justify their belief that the United States must adopt similar policies to preserve its share of trade in growth-promoting products. With the new trade theory now part of a debate that critically involves government policy toward high-technology industries and U.S. relations with Japan and the EC, it seems appropriate to review briefly some of the implications of strategic trade theory and the theoretical foundations on which it is based.

The enduring practical significance of Ricardian and other neoclassical trade models is the well-known normative case for free trade: everybody gains from specialization according to comparative advantage by trading goods they produce cheaply for those they cannot. Neoclassical trade theory considers the possibility of improvement in the terms of trade the lone justifiable circumstance for a policy other than free trade; in all other instances trade-policy intervention simply substitutes for (or adds to) existing distortions in the domestic economy, and so it cannot be a "first-best" option.

Though theoretically elegant, however, neoclassical trade theory is not free of empirical problems. One inconsistency with the theory is that in the post-1945 period an increasing proportion of international trade is not conducted along comparative advantage lines, but rather is of the intra-industry variety (for example, most industrial nations both import and export automobiles). Moreover, all neoclassical models begin with the same obligatory caveat: perfect competition and constant returns to scale. Yet many critics have long contended that, however useful this assumption for analytical simplicity, in some industries the

advantages of large size for capturing economies of scale promotes monopolistic or at least oligopolistic market structures. Where markets are imperfectly competitive, rents exist because they cannot be competed away by the entry of new firms. With the development in the 1970s of Chamberlainian models of imperfect competition, Krugman and a number of other economists began to develop a series of theoretical models of international trade in a world where prices deviate from marginal costs or where substantial external economies exist.

The significant finding of models of "strategic trade theory," as applications of the new trade theory have come to be known, is that under certain circumstances government trade and industrial policies can influence the success or failure of high-technology, "strategic" industries characterized by increasing returns to scale (or IRS, meaning that average costs decline with the scale of output), or which generate "spillovers" (technological knowledge transmitted to other industries). The implication, then, is that governments might want to pursue policies which ensure that these industries become concentrated within their own — rath-

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THE ISRAELI-PLO ACCORDS CHANGES IN POLITICS AND POLITICAL SCIENCE

Like the Berlin Wall, the cold war, the Soviet Union, or apartheid, the Arab-Israeli conflict once appeared permanent. It had achieved a taken-for-grantedness that seemed to overwhelm the possibility of change. Over the course of the past year, this rote acceptance has been shattered. With the inauguration of an Israeli-PLO peace process in the dramatic ceremony held in Washington on September 13, 1993, the Middle East became the latest region to experience the impossible.

Israel's agreement with the PLO, the recent Israeli-Jordanian protocols, and the possibility of a Syrian-Israeli settlement, offer social scientists a significant theoretical opportunity. The processes which led to the agreement and the changes still to come offer an occasion to rethink existing paradigms, test long-standing assumptions, and develop a research agenda capable of explaining not only the breakthrough of September 13, but the larger transition from war to peace, a process comparable in its implications and the scope of its consequences to the transition from authoritarianism to pluralism, or from command to market economy. Indeed, as a starting point, our conceptual understandings would be enriched by expanding the notion of transition to include processes of peace making alongside processes of political and economic transformation, incorporating as well the interactions among the three. Peace making is as much the domain of domestic political and economic life as of diplomacy, engaging issues of coalition building, the construction of social pacts, economic and social policies, development strategies, issues of identity and culture, institutional roles and relationships, religion and politics, the effects of regime type on the management of peace, and state-society relations in their broadest sense.

What are some of the concerns that might shape a new research agenda in comparative political studies of the Middle East? The possibilities suggested here are necessarily preliminary, if not speculative. The contours of an Arab-Israeli peace will take shape only over time, it will be many years before a generalized Arab-Israeli

peace can be regarded as consolidated, and the data to support new research agendas are only now being created. Moreover, there will be important areas of continuity across the pre- and post-agreement periods. Even at this early juncture, however, a number of research questions can be identified as early candidates for the increased attention of comparative social scientists.

One important area concerns precisely the effects of peace on states and state structures in the Middle East. War did not "make" the post-colonial states of the region in the fashion understood (and challenged) by scholars of Europe, yet the Arab-Israeli Conflict has had pervasive effects on processes of state building, and on the organization of relations between states and societies throughout the region. It has been a powerful force structuring state economies; shaping local cultures — establishing the conditions for social and political mobilization, molding the character of ruling ideologies; ordering inter-state relations, and defining the interactions between regional states and external powers. Similarly, peace making is unlikely to duplicate processes of war settlement and reconciliation which followed episodes of war in Europe. Middle East experiences thus hold out the opportunity to challenge and enrich existing theoretical understandings of war making and peace making alike.

The effects of the peace process on such regimes will be complicated further by its simultaneous occurrence with processes of economic and, to some extent political, liberalization. Transforming formal peace into substantive relations of peace is contingent

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THE DEVIL OF RUSSIAN DEMOCRACY

Like a hideous deformity that fascinates as it appalls, the rise of fascist Vladimir Zhirinovskiy in the Russian parliamentary elections of 1993 has dominated perceptions of Russia's transition to democracy. Led by "Sixty Minutes," the American entertainment media pursue Zhirinovskiy like star-struck teenagers. "Zhirinovskiy Watch" is a regular feature in *Time*. Headlines report meetings and nonmeetings with him when foreign dignitaries visit Moscow. The attempt by Russian democrats to meet Zhirinovskiy's threat with measures of mimicry that have deformed Russian foreign policy. Responding to Zhirinovskiy's nationalism and revanchism, Russian democrats have felt compelled to play the pan-Slavic card in Bosnia, and to demand better treatment for Russians in the "near abroad." Meanwhile, Russia's neighbors bang at NATO's doors, replacing dreams of peace dividends with nightmares of encirclement in the heads of Russian strategic planners. Domestic policy has been affected as well. Russia's Choice, the reformist party most closely associated with Yeltsin in the election, now finds itself in opposition; its leader, Yegor Gaidar, sits on the sidelines of government while its Western economic "experts" lecture other students at Harvard.

According to Yeltsin in his recent book, the devil is not Zhirinovskiy himself, but the Russian electorate, or a big part of it: "Zhirinovskiy revealed social, psychological, and moral wounds in our society whose presence we had not suspected."

If that is the explanation of Zhirinovskiy's electoral success, then prospects for Russian democracy are bleak indeed. Plummeting production, persistent inflation, inevitable unemployment, and unquenchable demands by industry, agriculture and the armed forces dominate the economic news from Russia. A potential civil war in the Crimea and eastern Ukraine threatens to ignite ethnic Russian nationalism. If Russians like fascism in 1993, they will like it more in 1995 and 1996, when parliamentary and presidential elections are next scheduled.

We disagree. Russian democracy is be-deviled, but the devil is not a taste for fascism. In an educated society grown weary of war and promises, Zhirinovskiy's lunatic threats and preposterous programs cannot endure scrutiny, and as we shall show, his electoral success is not evidence of policy support.

No, the devil is the manifest incapacity of Russian democrats to master the mechan-

ics of democracy. Zhirinovskiy is the incubus of Yeltsinite reformers, not the Russian voters. The democrats themselves created Zhirinovskiy with six election blunders. Four were tactical errors, correctable by simple experience. The other two are more basic, and they cause us to question whether Russian democrats will learn from their experience.

One blunder was Yeltsin's ban on the old opposition. By disqualifying parties and candidates that had too strongly supported the old parliament, Yeltsin left Zhirinovskiy and two emasculated communist groups in command of the opposing field. Disgruntled voters who had lost their taste for communism had nowhere to go but Zhirinovskiy. As a result, Zhirinovskiy's voice was unnecessarily amplified, and democratic reformers had no opponent to blame for past failures of policy. Seeking to amputate electoral opposition, Yeltsin only disfigured it.

Evidence abounds that swing voters, in the US and elsewhere, support or oppose governing parties according to the state of the economy; ignoring personalities and campaign promises, they punish governing parties when the economy sours. Thanks to the first blunder, the only way Russian

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voters could do that was by voting for Zhirinovskiy. Owing to the widespread impoverishment wrought by recent economic "reforms," the real news is not that Zhirinovskiy got so many votes (23 percent), but that he got so few — little more than half what neo-Nazi David Duke got in the 1992 Louisiana gubernatorial election.

The second blunder was Yeltsin's ban on criticism and polls. By forbidding candidates from criticizing each other, and by outlawing the publication of public-opinion polls a week before the election, Yeltsin sought to promote an issue-based campaign. Instead, the ban on criticism prevented any challenge to Zhirinovskiy's nonsense. Additionally, the ban on polls prevented voters from seeing Zhirinovskiy's parts as more than a harmless receptacle for protest ballots; earlier, published polls gave that party only seven percent of the vote, and Yeltsin himself only became aware of its strength only two days before the election. In a country long deprived of information and debate, it is self-defeating for democrats to legislate ignorance.

Blunder Three was extra air time for Zhirinovskiy. Because he supported Yeltsin's constitution — whose authoritarian provisions this presidential aspirant found particularly congenial — the media bureaucracy allowed Zhirinovskiy a disproportionate amount of air time to sell that document. Kremlin strategists failed to foresee how skillfully he would exploit the opportunity to sell himself as well. They should have. Zhirinovskiy had run before, and it is rumored in Russian political circles that he was a KGB media-manipulation expert.

The fourth blunder was more strategic. Fearful of communist strength outside of Moscow and Saint Petersburg, Yeltsin had half the Duma (the lower house of parliament) elected by proportional representation. This set an unintended trap for pro-reform politicians. Saint Petersburg Mayor Anatolii Sobchak, Deputy Prime Minister Sergei Shakhrai, and economic gurus Yegor Gaidar and Grigorii Yavlinski, each a presidential aspirant, seized the opportunity PR offers small parties by creating their own. With the democrats divided if not clearly distinguished, Zhirinovskiy captured more PR seats than any of them, 59 of the

229. The remaining half of the Duma was elected from single-member districts. In that half, the ardently Yeltsinite party, Russia's Choice, ran first, with Zhirinovskiy far behind other pro-reform blocs and even the communists.

A pure district system for the whole Duma might have helped regional communists, but it also would have shifted the spotlight from Zhirinovskiy to local personalities and issues. At the same time, almost any electoral system other than PR would have encouraged the creation of parties representing broader interests than collective farming, women's rights, industrial protection, and the nuances of economic reform. Seeking to exploit their geographically concentrated support, democratic reformers instead created a Duma that is as conservative and fractured as the old Congress of People's Deputies, one whose factions demand subsidies which portend bankruptcy.

Of the two blunders that reveal deep deficiencies in Russian politics, one is hubris. Armed with the pulpit of government, and fortified with the faith that the correctness of its own policies would be self-evident to all but unrepentant communists, Russia's Choice mounted a campaign devoid of any warmth, luster or sensitivity to the day-to-day concerns of most Russian — a campaign whose poster boy was the mushroom-like Gaidar. Marxists might assume the inevitability of their cause. Democrats cannot. In democracy as in baseball, it is inevitable that some team wins, but no team is the inevitable winner; victory requires hard and skillful play. Even today, reformers cast their appeal in economic jargon, presuming voters can do what professional economists cannot: discern the best economic policy in an environment of loud and conflicting demands.

The final blunder was the confusion of presidents with Tsars. Seeing himself as a chief of state above party politics, Yeltsin did not run for reelection or endorse any party. Pro-reform politicians were compelled to run under meaningless party labels, and pro-reform voters had no focal point for their support. Not that Yeltsin wished to be a figurehead. Before and after the election, he said his new constitu-

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CONFERENCES

QUO VADIS EUROPA 2000?

The Center for European and Russian Studies at the University of California, Los Angeles intends to organize a conference on the direction of Europe during March 16-18, 1995 in Los Angeles. The conference will focus on the present and future direction of post-Soviet and post-Maastricht Europe, with its core theme as the future relations between three overlapping geographic entities: Germany, the states of the European Union, and the entire continent.

Of the three major regions of advanced capitalism and modern civilization, only Europe has had its vistas substantially changed and enlarged in recent years. It is of vital concern that we understand the overall nature of these changes, and of the future trajectories they imply for the continent.

The end of the Soviet Bloc created unexpected and exciting possibilities for the integration of the European continent. The Maastricht process and the opening up of eastern Europe to democracy and capitalism also created new vistas for the European Union, potentially both "deepening" and "widening" it. Yet nation-states, nationalism, and great regional differences still survive. Furthermore, Europe contains one potential superpower, Germany. Its unification, its economic strength within the Union, and its large trade and investment with Eastern Europe have greatly enhanced its European (as well as global) role. The conference will discuss all three of these changes — and especially their implications for each other.

The sessions are as follows:

1. *The Depth and Width of European Integration.* This session will touch upon the general contours of Europe's future. How integrated will the new European institutions be? How "wide" will their boundaries be? How deep will be their penetration into social life?

2. *European Social Identities.* Is the nation-state weakening? Is a European identity resurgent, and will it replace or supplement national identities? What are the eastern and southern boundaries of "Europe?" Is "Europe" likely to be white and racist?

3. *The Maastricht Fiscal and Economic Process.* Will fiscal and economic integration deepen or will they falter? Is full currency union feasible? To what extent will the resulting system be dominated by Germany? Does the emerging economy have distinctly "European" characteristics?

4. *Forever the Political Dwarf and Military Worm?* Will the weakness of European foreign and defense policy continue? Will the continent remain dependent on American policy? Will institutions develop to cope with eastern turbulence?

5. *The Prospect for Democracy in Europe.* This session will cover two issue areas: (a) the "democratic deficit" within the institutions of the European Union; and (b) the prospects for further transitions to democracy, begun in southern Europe, continuing in eastern Europe.

6. *The Future of European Social Democracy.* Social Democracy is at present in crisis its traditional heartland of Europe, especially in its inner core, Scandinavia. This session will investigate the conse-

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REGIME TRANSFORMATION AND DEMOCRATIZATION IN COMPARATIVE PERSPECTIVE

INTERNATIONAL CONFERENCE UNDER AUSPICES OF JAMES S. COLEMAN CENTER

Participants: David Becker (Dartmouth College), Daniel Brumberg (Georgetown University), Tom Callaghy (University of Pennsylvania), Jyotindra Dasgupta (University of California, Berkeley), Larry Diamond (Hoover Institution), Giuseppe Di Palma (University of California, Berkeley), Barbara Geddes (Hoover Institution), Kobie Harris (San Jose State University), Arend Lijphart (University of California, San Diego), Karen Remmer (University of New Mexico), Pearl Robinson (Tufts University), Mustapha K. Al Sayyid (Cairo University), Thomas Schwartz (University of California, Los Angeles), Ivan Szelenyi (University of California, Los Angeles), Don Treiman (University of California, Los Angeles).

The world has witnessed dramatic changes in the past several years. Under the weight of economic crisis, internal and external pressures, the Soviet Bloc has disintegrated, and regimes in Africa, Latin America and parts of Asia have begun what appear to be definitive steps toward political liberalization. It has become clear that this global process of transformation poses a challenge to comparative scholarship. How are we to understand these events? How deep and how durable are the changes currently taking place? What are the most salient factors influencing regime change? In an attempt to explore these issues, the national resource centers at UCLA, with leadership provided by the James S. Coleman African Studies Center, collaborated in hosting a conference this past May 20-21 entitled "Regime Transformation and Democratization in Comparative Perspective: Asia, Latin America, Sub-Saharan Africa, the Arab World, and Eastern Europe."

The conference grew out of a desire to rethink what we know and do not know about the process of regime transformation in the non-western world. The basic assumption guiding the conference was that the pace, pattern and direction of regime change are shaped by international and domestic factors. Attention was to be given to the forces which stimulate or inhibit the

process, such as culture, ideology, development strategy, human agency, as well as the relationship between economics and politics in this transformation, and the factors that explain the transition from political liberalization to genuine democratization.

Participants included scholars from across the nation and abroad who addressed the following topics: 1) Theoretical Perspectives on Regime Transformation; 2) Economic Reform and Democratization; 3) Political Institutions and Regime Transformation; 4) Cross National Comparisons of Regime Transformation; 5) Social Movements and Democratization; and 6) Constitutionalism and Democratization.

The panels began with a penetrating critique by Karen Remmer ("Theoretical Perspectives on Democratization") of the current state of comparative theory, and its inability to address adequately what she sees as three key features of the recent events: the international forces which shape domestic politics, the enormous variation in societies which have undergone similar transformations, and the fact that democratization is happening in spite of economic decline. Giuseppe Di Palma followed with a provocative discussion of the impact of current changes on the state's role in the cre-

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DATABASES, ARCHIVES, AND WORKS-IN-PROGRESS

UPDATE ON PLANNING FOR THE COMPARATIVE POLITICAL, SOCIAL, AND ECONOMIC DATA ARCHIVING BOARD

In the next issue we may announce a contest to come up with a better name for this board! In the meantime, a committee of Robert Bates, David Laitin, Adam Przeworski (absent for this meeting), Richard Rockwell, Ronald Rogowski, and Phillips Shively met in Chicago on July 23 to develop a specific plan for a board. We will propose a national board of nine scholars from political science, sociology, and economics. The tasks of the board will be:

1. Advise the ICPSR on the archiving of aggregate data sets for the comparative study of politics and governmental capacity.
2. Develop proposals for new aggregate data sets; to a limited extent, develop data sets directly; scout for new data sets that are being developed, and encourage their "owners" to archive them accessibly; if possible, give small seed money grants, especially to graduate students.
3. Stimulate consideration of measurement issues, and the development of alternative measures.
4. Stimulate instruction in comparative data analysis, initially with a summer course offering at the ICPSR summer program.
5. Seek out international partnerships in data acquisition and organization. Initially, we would recommend four subcommittees of the Board, with each subcommittee consisting of three board members, associated graduate students, and associated scholars. Our four proposed subcommittees are:
 1. Review of existing aggregate data sets, especially older sets, with an eye to priorities for updating and for transformation into modern formats.
 2. Political and political/economic institutions, such as electoral laws, centralization/decentralization of government, executive and legislative rules for decisions, quasi-governmental corporations, etc.
 3. Extension of data sets to the sub-national level. (Can this, for example, help us to solve the small-N problem and increase variances?)
 4. Democratization, as a current target of opportunity for data acquisition.

The basic principles of the board are to be *rotation* and *consultation*. It is not our intent to create an *Academie Francaise* for comparative aggregate data, but rather to stimulate the development of a network of scholars to build and use aggregate data sets.

It is our sense that the field of comparative politics suffers from especially strong centripetal forces. Most of us have invested large sunk costs (in geographically-specific expertise, language, etc.) that tend to separate us from other comparative scholars rather than draw us to them; one might

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BOOK AND ARTICLE NOTICES

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PHILIP Q. ROEDER. 1993. *RED SUNSET: THE FAILURE OF SOVIET POLITICS*. PRINCETON: PRINCETON UNIVERSITY PRESS.

For comparativists who want to read one book on Soviet politics, *Red Sunset* is the one to read. For comparativists already well versed, *Red Sunset* should be next on the list. Roeder presents a truly comparative neo-institutionalist theory of the dynamics of authoritarian regimes, and applies it to the construction of a unified explanation for the rise and decline of the Soviet Union. Along the way he provides the most cogent single account of Gorbachev's reforms published to date, as well as a comprehensive overview of the development of Soviet politics from Lenin's day forward. Despite one significant reservation, the book represents the outstanding single volume on Soviet politics.

Roeder ultimately seeks to explain why Soviet institutions, capable at the beginning of such a profound transformation of Russian society, were ultimately unable to adapt to the new conditions engendered by initial success. According to Roeder, the reasons both for success and failure lie in a "constitution of Bolshevism" defined by a trait often found in authoritarian states: reciprocal accountability. This term refers to the choice and removal of top leaders by a selectorate composed of officials who themselves are appointed or removed by the top leaders. Roeder contrasts reciprocal accountability with the hierarchic accountability characteristic of democracies and absolutisms: in democracies top leaders are elected by voters whom the leaders cannot deprive of the franchise (at least not without abolishing democracy), while in absolutism the sovereignty of the monarch does not require the assent of the barons.

Over most of Soviet history (but not all) the selectorate was the Central Committee, which elected the Politburo and General Secretary, but itself comprised mainly the heads of large party and government agencies who were appointed by the Politburo.

Reciprocal accountability came into existence because peasants in Russia and the borderlands rejected Lenin and his colleagues' agenda of industrialization and urbanization. On the one hand, by precluding the population from voting to choose the national leaders, the communists increased their discretion to adopt policies contrary to popular preferences, while on the other hand, by making membership in the selectorate conditional on the continuing approval of the top leadership, the top communists disciplined their bureaucratic subordinates to impose the will of the leadership on the population. With his characteristic terminological adroitness, Roeder calls this original exclusion of the population "departicipation."

As a concept, reciprocal accountability is a big improvement on the alternative which to date has been the standard in the study of Soviet politics: the so-called "circular flow of power." According to this concept, the General Secretary used his powers over personnel to appoint the members of the Central Committee, who chose the delegates to the Party Congress, who

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reelected the General Secretary, who reappointed to the Central Committee those members who had demonstrated loyalty by their selection of delegates, and so on round and round.

Roeder shows both empirically and conceptually the poverty of the circular flow of power relative to reciprocal accountability. Not only the General Secretary, but also other powerful members of the Politburo manipulated the composition of delegations to the Party Congress. By the logic of the circular flow of power, the General Secretary could have been a sole ruler, but when the General Secretary was strongest (between 1937 and Stalin's death in 1953) he preferred to rule through the government and security police, virtually dispensing with the entire panoply of institutions — Party Congress, Central Committee and Politburo — which supposedly were crucial to the General Secretary's ability to rule. A Central Committee supposedly composed of the General Secretary's (at the time, First Secretary's) loyalists saved him in 1957 but expelled him in 1964, both times without reference to the Party Congress. All these facts have long been available, but not until Roeder has anyone presented a cogent institutional alternative to the circular flow.

Conceptually reciprocal accountability is superior because its explanatory variables are observable. In the circular flow of power, the unobservable personal allegiance of Central Committee members empowers the General Secretary to choose the policies he wants, and consequently whether the Soviet Union pursued reform or not depended on the unobservable personal predilections of succeeding General Secretaries. Placing himself explicitly among those political scientists who argue for the causal autonomy of politics, and who adopt an objectivist perspective explaining choice by constraints on actors, not by the actors' subjective understanding of their situation, Roeder uses the observable interaction between selectorate and top leadership to explain why the General Secretary could not use his very great powers to accomplish successful reform.

Roeder always notices the larger pattern. From Stalin's death onward, the Soviet leaders found themselves caught, as Ger-

trude Schroeder wrote, in a treadmill of reforms. Malenkov's new course in 1953, Khrushchev's destalinization, the economic *perestroika* of 1965, Brezhnev's economizing initiatives in the second half of the 1970s, Andropov's discipline campaign, and Gorbachev's democratization all foundered in the interaction between the Politburo and the Central Committee. In order to secure themselves against Politburo competitors, General Secretaries after Stalin built governing coalitions by offering policy payoffs to the heads of great bureaucracies assembled in the Central Committee. These bureaucracies had originally been delegated exclusive rights in zones of policy control designed for the task of industrializing an overwhelmingly peasant society. Loyal to the General Secretary only if the policy payoffs maintained their bureaucratic autonomy within those delegated zones of policy control, Central Committee members limited the General Secretary's options by the constant threat of defection to another Politburo member should the General Secretary try to empower any social groups (particularly the new intelligentsia of the urbanized society) not already represented in the selectorate. Consequently, although every General Secretary was fully aware of the destructiveness of the bureaucracies' continuing to carry out industrialization of an already over-industrialized economy, no General Secretary after Stalin was able to succeed in a coherent intervention against the bureaucratic selectorate.

Recognizing that his policy reforms would become mired in reciprocal accountability too, Gorbachev realized that only a constitutional reform could lead out of the bog. He took advantage of the circumstance after 1985, to which Boris Yeltsin himself has testified, that a majority of the Central Committee was temporarily unwilling to defect to any rival leader. This circumstance enabled Gorbachev to propose democratization and *glasnost* as means of undermining deparicipation. Following a strategy which Roeder cleverly labels "dynamic centrism," Gorbachev expanded the selectorate by matching each of his proposals granting new political rights to the Soviet population with a concession that

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would benefit a majority of the existing Central Committee at the expense of a minority. The enthusiastic response of the urban intelligentsia enabled him to present both his Politburo rivals and the Central Committee with an unpalatable choice between either defecting from Gorbachev, reimposing departicipation on the intelligentsia, and losing the benefits, or accepting incorporation of a second selectorate, the Congress of People's Deputies, which elected Gorbachev first as its chairman and then as Soviet President.

Despite its initial successes, this strategy was continuously vulnerable to defection by either the communist selectorate or the intelligentsia, and because the strategy necessarily consisted of half-measures intended to appease both wings of his coalition when each was demanding firm action against the other, defection by one side or the other was continuously likely. Gorbachev's 1990 failure to institute the privatization of state-owned industry promised in the "500 days" program triggered defection by the intelligentsia. Gorbachev tried to maintain himself by naming new figures, popular with the Central Committee, to top posts (Pavlov to the government, Pugo to the police, Yanaev as his vice president), but they joined with some of his earlier appointees to stage the coup of August 1991 instead. The risks built into the strategy of reform necessitated by the constitution of Bolshevism doomed the reform effort.

Roeder's ability to state precise hypotheses and provide systematic evidence builds the credibility of his argument. He integrates an extraordinary range of the extant literature on Soviet politics in one slim volume (253 pages of text), yet he also masterfully applies techniques of original documentary research where the existing literature does not provide the observations he needs. There are subtle, informative discussions of leadership succession, strategic policy, and resource allocation. The argument is further reinforced by evidence not included in the book but presented in his publications in a variety of journals, particularly his 1991 article in *World Politics* on the reasons for the failure of the Soviet response to ethnic diversity. Despite the powerful deductive argument and extensive

substantiation, I would suggest that Roeder has not considered one feature of the Soviet record that presents a major puzzle for his theory.

As Roeder notes when he argues that contestation among Politburo members provides the opportunity for the selectorate to restrict the discretion of the General Secretary, he and his rivals in the Politburo engaged in competitive differentiation of policy platforms. Roeder argues that Politburo members' public differentiation of individual reputations for advocating distinctive policy lines was aimed at the Central Committee, since under the putative constitutional norms of reciprocal accountability, no one below Central Committee rank could affect Politburo members' tenure in office — the "proximate goal" which he argues all their actions must pursue. If, however, the Politburo was targeting its messages on a Central Committee of three hundred members, they could have built their individual reputations for policy advocacy in closed meetings, or could even have circulated confidential memoranda. One puzzle for Roeder's model is therefore why the Politburo members differentiated their political programs in public. If Politburo members' speeches were intended to only offer concrete policy payoffs to a few senior bureaucrats justifiably assumed by Roeder to be hard-headed rationalists, it is even more puzzling why the Politburo members designed their speeches for the construction of symbolic political identities suitable for motivating audiences of many inattentive listeners. Why would Brezhnev or Gorbachev or Kosygin or Ligachev try to wrap themselves in Lenin's mantle, why would they try to personify the communist identity using rhetorical techniques comparable to those favored by electoral politicians addressing mass audiences? While affirming Roeder's thesis that Soviet institutions were shaped for the departicipation of the mass population, I have argued (*Public Politics in an Authoritarian State: Making Foreign Policy During the Brezhnev Year*, Ithaca, NY: Cornell, 1993) that going public by Politburo members with symbolic appeals offers evidence of their dependence for tenure in office, not merely

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**THOMAS CALLAGHY AND JOHN RAVENHILL. 1993.
HEMME*D* IN: RESPONSES TO AFRICA'S ECONOMIC DECLINE.
NEW YORK: COLUMBIA UNIVERSITY PRESS.**

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H*emmed In: Responses to Africa's Economic Decline*, is an interesting and thought-provoking contribution to the debate on structural adjustment. The phrase "hemmed in" refers to the situation of African governments who find that their options and resources are severely constrained due- to the interaction of weak states and weak markets, debt problems, and an unfavorable international environment. The editors deliberately aim to place the volume between the Afro-optimism of the World Bank, and the Afro-pessimism of others. Claiming a more realistic approach, the book attempts to address the political and economic causes and consequences of Africa's decline, the effectiveness of conditionality, the factors necessary to sustain adjustment, and whether pressures for democracy undermine or facilitate reform.

John Ravenhill begins with an historical overview of adjustment efforts, bringing up many of the themes which recur throughout the book. He emphasizes the complexity of issues involved and the accompanying uncertainty (for some) about solutions. Structural adjustment is increasingly recognized as necessary, but not sufficient, to solve the multitude of problems contributing to Africa's economic malaise. Broadening the focus for change, however, reduces the clarity of appropriate goals and priorities. Thus, a realistic assessment of what does and does not work is essential for progress.

Reginald Herbold Green provides an important overview of adjustment from a different angle. He outlines very clearly the differences and tensions between the perspectives, goals and policies of the IMF and the World Bank. This alone is a vital contribution to the debate, given the widespread tendency to conflate the actions and intentions of the two institutions. Green also gives a careful analysis of the extent to which each institution has learned from experience, and adjusted its approach to adjustment.

David Gordon examines the causes and dynamics of the debt problem, and argues that with the proliferation of policy targets, conditionality has become less effective. Through comparative regional analysis, he

highlights the failure of African governments to invest their foreign capital productively, and faults the international financial institutions (IFIs) for not providing necessary resources. He argues that over the long run, the political effect of donor conditionality is to destabilize incumbent clientelistic regimes.

Matthew Martin follows this with a comparative case study of conditionality in Ghana, widely considered a success, and Zambia, an example of reform efforts derailed. He finds the differences due to four factors: domestic politics, design and implementation of programs, capacity of the countries to adjust, and amount of foreign exchange provided by the IFIs. He calls for changes in the negotiating process, as well as increased flexibility and financing.

In a complex but insightful analysis, Naomi Chazan and Donald Rothchild examine the dynamic interrelationship between the political and economic spheres, as well as between the formal and informal spheres of political and economic power. Stressing the fluidity of authority in Africa, they argue that the locus of power is determined by the relationship between the social order, the state, and the regime. They argue that state decline, and at times adjustment itself, allowed for sources of accumulation apart from the state, which strength-

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ened civil society and democracy movements. They conclude that political stability and increased responsiveness of the state to civil society are necessary for economic progress.

Roger Riddell critically examines the current neglect of the manufacturing sector in reform efforts. Using historical and comparative regional analysis, he argues against both the radical pro-industry approach which characterized early industrialization efforts, as well as the "harsh withdrawal" approach which predominates today, making a very strong case that this neglect leads to industrial contraction without restructuring. He concludes by advocating benign intervention — selective protection without macro-level distortion — but he does not explain how to resist the political pressures which derailed previous attempts at intervention.

The next several chapters analyze responses to the decline of the agricultural sector. First, Sara Berry offers an historical analysis of the economic survival strategies pursued by African farmers, and traces the implications of these strategies for wider agricultural performance. Building on the literature about risk-averse behavior, she demonstrates how poverty and instability leads farmers to shorten their time horizons and increase their flexibility and liquidity. Berry argues that this does not necessarily create conditions for sustained growth, and may even contribute to further impoverishment.

Jennifer Widner then examines the political responses of cocoa farmers in Côte d'Ivoire to economic decline, discussing the economic and non-economic incentives for, and obstacles to collective action. She outlines the variation in levels of political activity found between villages, and presents a range of factors which contribute to this difference. In the end though, she finds little evidence that farmer's activism is likely to be organized into effective political pressure.

Jeffrey Herbst asks how to build political support for the long-term sectoral reform and infrastructure improvements required for agricultural revival. He examines three agrarian successes (Kenya, Zimbabwe, and Ghana), and their implications for other

countries. He finds that the factors in Kenya and Zimbabwe are unlikely to be duplicated elsewhere. Drawing on the Ghana case, he ends with a controversial call to the World Bank to recognize its political nature, and increase its local presence in African countries to help the farmers become a political presence.

This is followed by an in-depth case study of Cameroon by Nicolas van de Walle. He argues that reform was initiated in the interests of regime survival, but ultimately stalled because it was opposed by the very state elites who were supposed to implement it. He makes a compelling case that IMF and World Bank reforms are in general unsuccessful because they endanger rent-seeking opportunities, and therefore undermine the sociological foundations of the state, supporting the claim that true adjustment is unlikely without a regime change.

Michael Lofchie offers a detailed comparative analysis of Kenya and Tanzania, showing that policy choices have an enormous impact on outcomes. He argues that Kenya turned to inappropriate policies and conditions deteriorated, while Tanzania's improved policies did ameliorate conditions somewhat. He acknowledges that adjustment is not enough; recovery is constrained by infrastructure problems and a nearly complete absence of private sector actors. Despite the common notion that adjustment efforts are unpopular and therefore politically difficult, Lofchie argues that there are clear winners from reform, and that political opposition may not be as strong as feared.

Thomas Callaghy then examines the relationship between economic and political reform through a comparison with other regions, and then through analysis of three case studies (Ghana, Nigeria, and Senegal). He concludes that political reform could make economic reform more difficult, and then suggests how the sequence of reforms can affect the success of the efforts.

The volume ends with an assessment by Callaghy and Ravenhill of how "hemmed in" Africa really is. Although claiming a realist position, in the end, the editors' (particularly Callaghy's) conclusions are far more pessimistic than those of the contrib-

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HANDBOOK OF CENTRAL AND EAST EUROPEAN POLITICAL SCIENCE. 1994. COLCHESTER, ENGLAND: EUROPEAN CONSORTIUM FOR POLITICAL RESEARCH

Two main problems confronted social scientists who did research on the former communist states of east and central Europe and the Soviet Union: the paucity of data, and the lack of links, both informal and formal, with scholars and institutes in these countries which characterize the study of western Europe. With the fall of communism, these problems, while still existent, have begun to recede. New data sets on the ex-communist countries are springing up rapidly (see *AFSA-CP* 5:1, Winter 1994, for a review of an index of some of these new data bases). Also, bridges between scholars and institutions on both sides of the old Iron Curtain are beginning to be forged.

To aid these new civil engineers of political science, the European Consortium on Political Research has produced the *Handbook of Central and East European Political Science*, a roadmap to the people and organizations of the discipline in the region. It covers eighteen countries (Albania, Belarus, Bulgaria, Croatia, the Czech Republic, Estonia, Hungary, Kyrgyzstan, Latvia, Lithuania, Poland, Romania, Russia, Serbia, the Slovak Republic, Slovenia, the Ukraine, and Yugoslavia).

In an attempt to be comprehensive, and to be inclusive rather than exclusive, it defines its terms as broadly as possible. The aim is to include all information which may be useful for political scientists. At the moment academic change can be rapid in the region, and the boundaries between political science and related disciplines are less clearly drawn than in the West. Therefore, the *Handbook* covers all the subfields within political science, and also spreads over into law, economics, sociology, psychology, history, journalism, and demography. It includes both public institutions, such as departments and centers of universities and other publicly-funded organizations, as well as private and/or profit-making organizations doing research in politics.

The information is organized by country. Within each country section, national-level institutions come first — i.e., the

Academies of Science and national political science associations — followed by departments, centers, institutes and agencies in alphabetical order. The table of contents list each institution by country, and there are alphabetical indexes for both individuals and institutions in the back.

Each entry includes:

1. the name of the institution, address, phone number, fax number, and e-mail address;
2. name and address of contact person;
3. publications — book series, journals, newsletters, occasional papers;
4. a brief description of the institution;
5. members of the institution, and their main teaching and research interests.

The *Handbook* is available in hardcover from the European Consortium on Political Research for £10/\$15 for ECPR members and £35/\$55 for non-members. For more information, contact:

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ANNOUNCEMENTS

COMPARATIVE POLITICS AWARD DEDICATED TO THE MEMORY OF GREGORY LUEBBERT

On behalf of the American Political Science Association's Organized Section in Comparative Politics, the officers of the Section would like to express our appreciation for the contributions by the colleagues, friends and family of Gregory Luebbert to endow an annual research award in his memory.

It is very rare that a junior scholar emerges so quickly to a position of recognition and admiration in an academic field, yet this was something Gregory achieved in his career. His book, *Liberalism, Fascism or Social Democracy* was the recipient of the Section's first award for outstanding scholarship in the comparative politics field — the only such award in the profession. Thus, it is very fitting that we can use these funds to establish the annual Gregory Luebbert Award for Research Excellence.

This year's recipient of the Luebbert

Award is Robert D. Putnam, for his book, *Making Democracy Work: Civic Traditions in Modern Italy*, published by the Princeton University Press in 1993. The Award will be presented at the Section's meeting at this year's American Political Science Association Convention in New York.

Words cannot express the feelings of losing a valued colleague, but we hope this award will keep his memory alive, and bring distinction to other scholars who have reached exceptionally high levels of achievement.

NOMINATING COMMITTEE SELECTS CANDIDATES FOR SECTION EXECUTIVE COMMITTEE

Just as roses bloom every summer, so too does the Section's Executive Committee Nominating Committee. This year's Committee was composed of David Laitin (University of Chicago, and President of the Section), Edmond Keller (University of California, Los Angeles), Susan Shirk (University of California, San Diego), James Caporaso (University of Washington), and Gerardo Munck (University of Illinois). The Committee's responsibility is to select nominees to fill the annually vacated slots in the Section's Executive Committee.

The Committee's nominees are Frances Rosenbluth (Yale University) and Catherine Boone (University of Texas, Austin). Their nominations will be put to a vote at the Sec-

tion's business meeting at this year's APSA Convention in New York. According to Section By-Laws, other nominations may be offered from the floor.

Different schools presuppose different models of epistemology and science, making important differences about subject matter, theoretical approaches, and technical methods.

Contests between various philosophies of science and related political theories is good for intellectual freedom and advance in a field like comparative politics. Thomas Kuhn's discussion of normal science brings out the closed, almost oppressive character of science operating under a paradigmatic regime, and contrasts it with the more open, contestable, and creative periods of revolutionary and pre-paradigmatic science. The benefit of normal science is progress in advancing the implications of the reigning worldview. Yet by unifying scholars and enforcing a common picture of reality, the role of data in testing a paradigm is derivative. To overcome this, and to help make diversity in experience intelligible, various ways of knowing may prove useful.

This leads to my third point. The crucial problem for comparative politics has to do with gaining control over our tools of political analysis. Our models picture the political world differently because different epistemological presuppositions entail different conceptions of science, methodological technologies, topics, and data. Pluralists' presuppositions picture a society-centered and responsive polity, facilitating a focus on autonomous agents moved by

subjective interests to engage in associative behavior. State-centered theorists' antecedent epistemology establishes institutions as objective actors in their own right, with society subordinated to state imperatives. To gain control over these and other methods of analysis, comparative analysts need to develop an awareness of how antecedent prejudgements and prejudices fix grounds and entail ends, to see the consequences of theoretical and technical instruments, and to make them more accountable to moral purposes through the use of counter-argument and contradictory data.

In short, it is a mistake to think that comparative politics will, or should become a paradigmatic science like physics, particularly given that it must strive to give meaning to diverse and often incommensurable experiences. Still, comparative politics can advance to the extent that it recognizes that ideas are tools, and that the foundations of knowledge are themselves only conjectures linking conditions and empirical effects. This means moving beyond commitments to foundationalist concepts of science and political theories, and focusing on what each has to offer in solving political problems. Rather than advocating a unifying paradigm, the need to develop a community of critical inquirers intent on gaining control over the empirical consequences of ideas rather than the triumph of first principles.

**Wolfe (cont.
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we might seek one or two positions in our departments for comparativists who wish not to be linked to a single area. In fact, those positions should be open to comparativists whose empirical enterprise did not involve field work at all, but whose work solved important theoretical problems. Alternatively, we might ask candidates for job positions whose area of expertise is somewhat different from the stated geographic need of the department if they feel comfortable teaching undergraduate courses covering politics and government in regions in which they are not then researching. Our teaching and advising duties need not be

exactly commensurate with our current research program. We might also take advantage of the breadth of scholars taking the first path in accepting gaps in our regional coverage for courses as they give new courses in the regions in which they are gaining expertise, so that students and teachers are both learning the nuances of the region at the same time. This can be an exciting experience for undergraduates, and a useful one for comparativists seeking to develop an interpretive feel for a new region. Whatever strategy a department takes, it should make greater accommodation to

**Laitin (cont.
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**Thomas-
Woolley
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ation and fruition of citizenship ("Democracies as Nation-States: Some Notes on the State's Enabling Roles, Past and Present"). He concluded that the most successful democracies are based on a "welfare compromise" which reconciles the reproduction of capital with the reproduction of popular consent.

The next panel was on economic reform and democratization, and consisted of Barbara Geddes, who presented a strong and cogently argued critique of the common wisdom that economic liberalization does best when it precedes political liberalization ("The Effect of Economic Liberalization on Labor and Democracy"), and Tom Callaghy, who presented a thought-provoking analysis of the issue from a regional-effects perspective rather than using the typical comparative case-study approach ("Reform in a Weak Neighborhood: Economic Change and Democratization in Africa").

The panel which followed addressed the topic of political institutions. Ivan Szeleynyi presented his work on a comparative three country study (Russia, Poland, and Hungary) which examined the extent to which the far-reaching changes in the political system have altered the personnel of the elite ("Changing Patterns of Elite Recruitment in Post-Communist Transformation"). Thomas Schwartz, presenting a paper he wrote with Peter Ordeshook, ("Democratic Reform and the Mechanics of Democracy") offered a formal model of the process of constitution-making. Arend Lijphart then provided an intriguing analysis of Indian democracy in light of consociational theory, ("The Puzzle of Indian Democracy: A Reinterpretation") which addresses democratic institutions in ethnically and linguistically divided societies. He concluded that India is not an exception to consociational theory, and if democracy is to survive there, it must return to the power-sharing of the first two decades of independence.

The conference continued Saturday morning with a panel on cross national comparisons, in which Larry Diamond strongly argued that despite perceptions to the contrary, progress toward democracy in Latin America as a whole has stagnated, with gains more than offset by setbacks.

Through careful comparative analysis, he outlined the requirements for meaningful consolidation and deepening of democracy, emphasizing political institutionalization in particular ("Democracy in Latin America: Degrees, Illusions and Directions for Consolidation"). Don Treiman then offered an exploration of preliminary data from a comparative study of elite power in regime change, building on his research on socio-economic structures and elites ("Did the Transformation to Post-Communism in Eastern Europe Restore the Pre-War Class Structure?").

The next panel addressed the role of social movements, and consisted of two pieces on the relationship between Islam and democracy, and a discussion of national conferences in Africa. Daniel Brumberg began with a stimulating inquiry into the scope for transitions and elite pact-making within utopian ideologies in general, and Islamic states in particular ("Utopian Ideologies and the State: The Case of Islamic Movements"). Mustapha K. Al Sayyid followed with a critique of the presumed incompatibility between Islam and democracy, and an assessment of the Egyptian case ("Islam and Democracy in Egypt"). Pearl Robinson then analyzed the composition and dynamics of civil society in francophone Africa, and suggested that the national conference phenomenon emerged in part through a transference of ideas from the French Revolution among African elites ("Civil Society and the National Conference in Francophone Africa").

The conference concluded with a panel on constitutionalism. Jyotindra Dasgupta, introducing the concept of "developmental federalism," addressed the role of constitutionalism in supporting federalizing institutions in India, not just as a mechanism to balance government, but as a means to enable development ("Democracy and Developmental Federalism: Exploring the Role of Constitutionalism as a Constructive Adventure in India"). Kobie Harris followed with a discussion of constitutionalism's historical role, and possible current applications in Africa ("Constitutionalism: The Genetic Code for Restoring the African Polity"). David Becker then offered a

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quences for Social Democracy of (a) the continuing internationalization of advanced capitalism; (b) the fiscal rigors of the Maastricht process and of Bundesbank policy; (c) Scandinavian entry into the European Union; and (d) the entry of poorer ex-socialist countries into the Union.

7. *The Eastern Marches.* Where will the eastern frontiers of Europe stabilize? What are the prospects of the ex-Soviet Bloc countries, either as fully European countries, or as dependent societies and economies?
8. *Will Europe Finally be Germanized?* To what extent will Germany remain the dominating presence in Europe? How

will Germany use its power? Both federalism and the stalling of integration are sometimes presented as rival alternatives to an integrated Europe dominated by Germany — is this correct?

Speakers will include Jürgen Habermas, Massimo Cacciari, Karl Kaiser, Jose Maria Maravall, Tadeusz Kowalik, Jrgen Kocka, and Anne-Marie le Gloanne.

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**Mann (cont.
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say, we have some affinity with the Tower of Babel. It is in order to take a small step towards overcoming this that we propose this Board as an institution to mobilize the community in the production of a collective good.

An important part of this mobilization will be broad and frequent communication, and that is part of what we envision for the Board. Another important part is to work with graduate students and young scholars, to help build links among them at the be-

ginning of their careers. Finally, we hope that increased work among the community on the methodology of measurement and comparative analysis will help to set in motion a virtuous circle of training, discussion, and replication, leading in turn to further methodological development.

Of course, the Board will require a base of funding support, and securing that is the next step. We can call forth spirits from the deep — but will they come?

**Shively
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compelling argument about the significance of squatter's property rights for constitutionalism, and the functioning of democracy ("Citizenship, Equality and Urban Property Rights in Latin America").

The wide range of perspectives and approaches which the participants in the conference brought to bear on the questions of democratization underscores the complexity of the issues involved. Common assumptions about causality and even the current status of transitions were frequently challenged. Clearly there are no simple answers and change is occurring so rapidly

that today's progress toward understanding will have to be reexamined in light of tomorrow's events. Yet we are left with a deeper insight into many parts of the puzzle and a stronger appreciation of the distinct challenges which the current events pose for the field of comparative politics.

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**Thomas-
Woolley
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**Anderson
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on a few hundred members of the selectorate, but on the mass of *nomenklatura* bureaucrats who imposed the leaders' will on a passive population.

This evidence erodes one of Roeder's core ideas — reciprocal accountability of leaders to a selectorate whose members they hire or fire. For while the Politburo could certainly promote or remove any individual bureaucrat, it could not dispense with the services of the *nomenklatura* as a whole. Politburo members needed to be responsive to bureaucrats' preferences just as (but not as much as) electoral politicians are to the preferences of voters. Roeder's other core idea — the need for authoritarian regimes to maintain popular departicipation — becomes the more powerful explanatory variable in explaining the institutional peculiarities of the Soviet Union, and the most important question becomes the one Roeder systematically eschews. When at the beginning of the volume he opts for strict objectivism, explaining behavior of actors only by external constraints, he gives cogent reasons for preferring this strategy to intentionalist concepts like ideology, political culture, and learning. Yet as all institutionalists are compelled to do, he exaggerates the explanatory power of external constraints by neglecting the paradox of participation. In order to enforce departicipation on the populace, the communist leaders needed to secure the participation of the *nomenklatura*. At this point they encountered the problem of collective action, and if the *nomenklatura* in fact consisted of the strict rationalists Roeder posits, not even selective incentives would be

capable of overcoming the obstacles to monitoring that break down rationalistic accounts of institutional cohesion. To explain institutional action, one needs not to revive the unobservables of intentionalism, but instead to investigate the processes by which the affiliative functions of language cumulate individuals into collective identities. If the crucial question is how the Soviet leaders overcame the paradox of collective action by constructing an activist identity for the *nomenklatura* in opposition to that of the pacified populace, then the explanation for Gorbachev's failure to preserve the Soviet Union must be sought in his effort to extend the activist identity to the populace without compromising the collective identity of communists built on denial of popular capacity for conscious action.

While I therefore think that Roeder has left one key puzzle unresolved, that hardly negates the value of his contribution. Although no one has yet provided a coherent rationalist account of participation in institutions, no one has proven it to be irrational either. If his book has a shortcoming, its weakness is common to all objectivist accounts, and readers committed to institutionalism will not be troubled by it. Those who are not will still find the argument stimulating, instructive and persuasive. As Roeder writes with exceptional lucidity, even though the book is not intended to be a text, it would make a fine one for advanced courses not only about Soviet and post-Soviet politics but also about the phenomenon of authoritarianism in general.

**Heydemann
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of understanding and explaining the transformation represented by the still incom-

plete transition from war to peace among Israel, the Palestinians, and the Arab states.

**Chase (cont.
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itiveness in high-technology sectors simply diverts the economic policy debate with spurious arguments that lack firm grounding in theoretical economics. Thus, Krug-

man's essential message is that while economists work to define strategic trade theory's proper scope, policy makers would do well to recognize its limits.

tion would endow him with enough power to rule. What he failed to appreciate is that the power of an elected president is mostly informal. It comes more from skillful leadership than from constitutional provision: as the only nationally elected official, the president is in a unique position to speak for a nationwide constituency, to marshal public opinion, to coordinate agreement around policy goals, and to point voters towards supportive parliamentary candidates.

Yeltsin is well aware of his unique position. Yet so far, he has interpreted that position as a license to act rather than an opportunity to lead. This interpretation is reflected in the new constitution. Misunderstanding the mechanics of democratic leadership, the Kremlin gremlins who wrote the document opted for crude instruments of presidential power: the president may appoint and fire ministers without legislative consent, legislate whenever the law is silent, dissolve the Duma as well as veto its acts, abrogate laws he deems unconstitutional, and in default of any contrary law, appoint the upper house of parliament and determine the rules for reelecting the Duma and himself. Yeltsin might use these powers wisely, but other presidents might not, and skillful ones do not need them; the best of ours did not.

The details of the new constitution invite crises. Parliament can legislate, but the president can rule by decree. The president can veto parliamentary acts, but parliament can veto presidential decrees. The president can hire and fire ministers, but parliament can vote no confidence in the government. The president can claim a national mandate, but thanks to national PR, so can parliament. In the US and elsewhere, overlapping powers often create compromise. However, in a country in which even basic property rights await legal definition, they are as likely to create conflict and crisis. Russians often argue that stronger, more stable parties would help; they often fail to appreciate that parties are creatures of electoral and constitutional rules: good parties are not created by mere design or by poorly designed rules.

Russian democrats can blame themselves for Zhirinovsky's success. In a way that is good: not only is fascism not inevitable, but it is possible to learn from mistakes. George Washington's first run for the Virginia House of Burgesses was unsuccessful. Unlike his opponent, Washington had not brought a cask of rum to the polls for his supporters, but he learned. The next year he brought two casks and won.

Russian reformers have much more to learn about the mechanics of democracy and not much time to learn it; the next elections are scheduled for 1995 and 1996. So far, Yeltsin has shown at least two signs of Washington's learning: he has fired some of the officials directly responsible for campaign mismanagement, and he has begun to match Zhirinovsky, cask for cask, with spirited expressions of concern for the plight of the average Russian.

We see reason for skepticism, however, in the sheer extent of past blunders in the legal and constitutional cement with which some of those blunders have been hardened, in the apparent inability of reformers to coordinate their strategies and suppress their egos, in Yeltsin's continuing insistence that the magic bullet of reform is to be found in authoritarian constitutional devices more than the informal powers of a democratic leader, and the current effort to muffle political debate with a "Civic Accord."

We see greater reason for skepticism in the inferences drawn, by Yeltsin and other Russian democrats, and by their worried friends and wary neighbors, to Zhirinovsky's electoral success: that an antidemocratic devil possesses a fair portion of Russian society. Fortunately, the evidence reveals no such devil. Unfortunately, Russian democrats are bent on exorcising one instead of seeing what the evidence does reveal: their own failure to master the mechanics of democracy.

The man President Clinton called "Russia's best hope for democracy" may be that, but only if he quickly starts to cultivate institutions and practices in which politicians, inevitable including critics of his own policies, can learn the game of democracy.

**Ordeshook
and
Schwartz
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Heydemann
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on more than periods of confidence building and the design of demilitarized zones. It hinges as well on the capacity of regimes in the region to manage the complex of pressures produced by multiple, simultaneous transitions. As in much of the world, identifying politically manageable strategies of economic reform has become a preoccupation of governments throughout the Middle East. Adjusting these strategies to take the peace process into account adds an important additional strain. The interactions among these processes deserve attention from comparativists, who have already generated considerable research on the dynamics of simultaneous political and economic transformations across a range of methodological approaches.

The states engaged in peace making will also contend with issues relating to economic conversion and the transformation of military-industrial complexes to non-military uses. Anticipating diminished needs for military manpower, governments will be pushed to cope with the economic and social consequences of military build-down, from rising unemployment to discontent among the armed forces. How to design social, economic, and political policies to capture the economic benefits of peace — while ensuring adequate security — has not been an issue-area of much consequence for the Middle East, and could benefit from the application of insights and cautions gained through the study of other regions.

The issues mentioned thus far tend to assume the presence of a state. For the Palestinians, however, the immediate consequences of the transition from war to peace will develop in the absence of formal state structures, even recognizing that the September 13 agreement may lead to the eventual establishment of an independent Palestine and/or to a redefined relationship with Jordan. Indeed, the experiences of Palestinians during the interim period covered by the agreement and beyond are likely to fit well with other cases of state-building brought about through civil conflict, decolonization, or the disengagement of a ruling authority, and should be seen as a laboratory for comparative study of state formation in process. In grafting an exile leadership structure onto the well developed

political movements of the West Bank and Gaza, the experience of the African National Congress may offer useful parallels for examining the processes of transformation underway within the Palestinian national movement. The experiences of other African states — Mozambique, Zimbabwe, and now South Africa — may provide useful lessons for understanding the problems associated with integrating former combatants into an already densely organized society. Central American experiences may also provide useful, if not reassuring, reference points for understanding this process. Israel's disengagement from the Occupied Territories may be explored through comparison with the experience of the French in Algeria or, more pessimistically, with the British in Northern Ireland. Finally, it is to be hoped that the transition from war to peace will not only create new research agendas, but will reshape the research environment as well, slowly overcoming the considerable constraints on the conduct of research confronting scholars from the region as well as scholars who study the region from elsewhere. Peace is not likely to produce an immediate change in the outlook of governments toward research and researchers, nor persuade them immediately to invest in rebuilding the intellectual and research infrastructures of the region. Yet as the circumstances created in response to decades of Arab-Israeli conflict give way to the gradual consolidation of peace, obstacles to research in the Middle East may well diminish.

The passage from war to peace in the Middle East will not be completed quickly or smoothly. Such transitions are rarely linear. More often they are messy and discontinuous, subject to setbacks, retreats, or reversals. Messiness, however, should not diminish the theoretical significance of the transition, or its value as an organizing framework for comparative research. Aspects of life in the region will not be substantially altered by peace. Yet much will change, and these changes require that social scientists ask new questions, re-examine conventional wisdoms, explore new methods and approaches, and promote the development of research agendas capable

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er than their neighbor's — borders, or which keep declining IRS industries afloat. This in turn gives rise to myriad strategic issues (first-mover advantages, so-called "commitment technologies," to name the most prominent) which the international economics literature has sought to puzzle through despite the frequent difficulty of finding equilibrium outcomes.

In the process, these models cumulatively issue a strong challenge to the classical case for free trade. In the new international economics, trade is based not only on comparative advantage, but also on national advantages that result from historical accidents which then persist due to the advantages of large scale or accumulated technical know-how. It follows that if comparative advantage is not simply inherited from factor endowments but also influenced by first-mover advantages, learning by doing, and innovation, then government policy can affect patterns of comparative advantage by promoting the development and expansion of high-technology industries which produce external economies (spillovers), for instance by making credible strategic commitments to promote the entry of domestic firms and deter the entry of foreign firms into initially unprofitable industries. Moreover, if size confers strong advantages, appropriate government intervention might enable domestic firms to capture international rents from foreign rivals. In these and other scenarios in which technology is endogenous, susceptible to government policy, and industrial subsidies and trade protection can confer persistent national advantages, the implication is that industrial and trade policy intervention can be welfare-superior.

Thus, the new trade theory carries normative implications that many observers of international trade and trade policy, especially those sympathetic to government activism, intuitively hold to be true yet which neoclassical trade theory rejects. It implies that some goods are more growth-promoting than others — in other words, to invoke a familiar debate, it really is more important to make computer chips than potato chips — and shows that governments, at least in principle, *can* intervene to promote the 'right' industries. Models of strategic

behavior, in a simple-minded way, resemble to many in the policy world the closed-market behavior of Japan (the cellular telephone dispute being only the most recent alleged transgression) and the Asian NIC's, or the activist industrial policies of France and the EC (with Airbus a favorite target). As Krugman notes in an early volume devoted to interchange on strategic trade policies between academics and government officials, many of the pioneers of strategic trade theory have been "surprised and perhaps worried at the places they find themselves cited." Protectionists, managed traders, and advocates of a business-government partnership alike, Krugman asserts, embrace the theme of competition between nations carried out through industrial and trade policies, and only selectively absorb the implications of strategic trade theory into their policy agendas. Now that many of the apostles of strategic trading occupy a position close to the President's ear, the theme that America is falling behind, and must adjust government policies as diverse as health care, R & D subsidies, and tariff retaliation to compete with its international rivals has become official rhetoric, if not always policy.

But far from a call for a sweeping industrial policy or government-managed trade, what models of strategic trade policy demonstrate is that trade intervention can be useful in some instances and not others; the task for policy prescription is to identify the "empirically relevant cases." Initially there was little effort to translate strategic trade theory into empirical enterprises because the analytics of predicting the outcomes of firm-firm and firm-government strategic interaction frequently were too difficult. As the accumulated empirical studies of strategic trade theory increasingly show that strategic sectors are difficult to identify and successful strategic policies difficult to craft, the initial bursts of enthusiasm over the uses of strategic trade theory have given way to guarded caution in more recent volumes.

In consequence, Krugman and others have issued several reasons not to use the new trade theory as an excuse or a model for government intervention. For starters,

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**Thomas-
Woolley
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uting authors. The editors review the other chapters, showing repeatedly how Africa is caught in the vice of weak states and weak markets. They claim the current prospects are grim and call for greatly reduced expectations of change over the long term.

The volume repeats well-known complaints such as the fallacy of composition and the failure of IFIs to provide adequate funding, but the case studies provide further empirical evidence in support of these common critiques. In addition, the authors articulate some less familiar positions such as Lofchie and Riddell's contention that adjustment policies cause de-industrialization.

Although offering a sound critique of presently pursued paths, this collection does not offer much in the way of viable alternatives. Several authors take up discussion of the conflicting political and economic incentives leading to what Gordon refers to as a "suboptimal mix of partial measures," but no one addresses how to alter the incentive structure. Although not actually posed as a solution, Gordon argues that fur-

ther marginalization of Africa is not necessarily bad. In the other extreme, Herbst's bold call for more direct intervention by the World Bank would be hotly contested by African governments, resisted by the Bank itself, and is judged by the editors as unlikely to succeed even if implemented.

The clarity and scope of the writing makes the book accessible to the beginning student of political economy in Africa, while the comprehensiveness of data and depth of analysis make the book a valuable resource for those more familiar with the literature on the subject. Although at times covering familiar ground, the editors are consistent in their effort to carve out a more realistic assessment of recent events in Africa and to discover any lessons which may be learned about change and the obstacles to reform. Understanding what is wrong does not guarantee knowing what is right, but it is a crucial place to begin. As the editors themselves assert, "Learning lessons and being able to apply them are different things."

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there are few practical guidelines for determining where spillovers might be large. In addition, empirical studies typically conclude that the number of industries subject to increasing returns are so few, and the potential monopoly profits so small that the national welfare gains of strategic trade policy are probably insignificant when weighed against deadweight losses.

On the other side of the ledger, Krugman contends, the potential costs of a misguided strategic trade policy are much larger: since export promoting policies bargain off capital and labor from industries less favored by government policy, causing them to contract while targeted sectors expand, strategic trade policy could favor the 'wrong' sectors, reducing national income by adding to the costs and hurting the profitability of established industries. As a result, Krugman concludes, IRS industries are so few and so difficult to identify, and trade of so little significance to the U.S. econo-

my, that government involvement in industrial policy is inevitably counterproductive. He accordingly calls for a limited program of subsidies to selected industries (about \$10 billion — more, ultimately, than Clinton's investment plan) but unequivocally rejects managed trade or import protection for the purposes of export promotion.

More generally, in Krugman's view, debates over how to use trade policy to raise living standards represent an excuse to blame America's economic woes on perceived unfair trade practices abroad. What really matters, he argues, is domestic productivity in the services industry (which is seventy percent of U.S. output but only twenty percent of its trade), not competitiveness in export sectors — especially because trade composes less than fifteen percent of US GDP. With trade still such a small proportion of total output, Krugman concludes that all of the talk about compet-

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tant empirical comparative research is to the generation of abstract explanations of otherwise puzzling phenomena (i.e., to theory).

Once the struggle for intellectual hegemony is seen as a political (and not simply "scientific") contest, it is not at all surprising that two of the smartest generals in the camp of the rational choice deductivists, Kenneth Shepsle and Barry Weingast, lost little time in trying to exploit Laitin's concessions to the hilt. In their response to his comments, published in the Winter 1994 issue of this newsletter (*APSA-CP*, 5:1), Shepsle and Weingast graciously accept Laitin's criticism of "theory" divorced from empirical work and welcome his suggestion that comparativists provide "theorists" with encouragement, interesting puzzles, and constructive criticism. However, the intellectually and politically crucial aspect of their essay is its unargued, presumptive use of the term "theory" to mean, as Laitin allows them to, the specific kind of deductivist formal modeling of rational choice propositions that they employ in their work. Indeed the terms "theory" and "theorists" (not "pure theory" or "rational choice deductive theorists") are used no less than seventeen times in their short essay (including three references to "positive theory[ists]") as generic descriptors whose *actual* referents (in the context of their argument) are "rational choice models."

Indeed Shepsle and Weingast go well beyond Laitin's formulations, equating their particular research program with the very act of "abstract reasoning," and holding out the study of the American Congress as a "superior exemplar for comparative politics." This last move dramatically reveals the inappropriateness of deductivist rational choice theory for the study of political systems where institutional context is highly problematic, where fundamental issues of political legitimacy are not hegemonically solved, where elites are unwilling to accept defeat within the rules of formal legal institutions, or where culture surrounds po-

litical competition with meanings less congruent with the psychology of the market. From the viewpoint of most comparativists, the American Congress is a special case in the study of political institutions, not a generalizable exemplar.

This point about the contextual specificity of Congress stands wholly apart from another crucial question that must be raised about the Congress-based research program that Shepsle and Weingast suggest as a model for all of comparative politics. I, for one, have serious doubts about whether data from rational choice studies of the American Congress have ever been, or can ever be used, to test rational choice models, or set limits to their utility, or whether the accumulation of these studies is driven instead by a never-ending "Glass Bead Game" of theorems and corollaries elaborated to express in formal terms a reality that is not more complex than the models developed to describe it.

I am not here suggesting that the formal modeling work of rational choice theorists has not and will not continue to contribute to our understanding of a broad range of political phenomena. Nor am I suggesting anything inappropriate about the use of political techniques to advance a scientific agenda. I do wish to remind comparativists that theory can be developed at different levels of abstraction, and by using different kinds of techniques. It is exceedingly dangerous, as the state of contemporary Economics demonstrates, to accept too narrow a definition of "theory" as hegemonic. I also wish to call attention to the way in which the discursive strategy used to advance formal rational choice modeling as a particular view of our discipline, and the dynamic processes which institutionalize and deinstitutionalize preferences at the hegemonic level, are understandable only by mobilizing techniques of analysis and *theoretical* resources which the assumptions of rational choice theory would define out of existence or drive to the margins of our field.

**Lustick
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**Laitin (cont.
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the skill development of its comparativist faculty, however useful it might seem to be to have career-long incumbents covering a particular region.

The grave issue of comparativists retooling for their second research projects has implications for the way political science departments advertise new positions, develop their curricula, and invest in our careers. I would hope that departments, especially those expecting a life of continuing inquiry by their faculty, would open the possibility for development along both paths. Our current practice, I fear, not only favors the second path — that of examining a new theoretical issue in the region of

expertise — but underestimates the retooling costs that are necessary to engage in continuing serious research along that path.

Furthermore, young scholars who are committed to following the first path, and those whose work does not involve area expertise at all, often find themselves facing advertisements for job positions that are too closely tied to areas for them to even want to apply. Our very advertisements thereby project an extremely limiting model of career development. The issue of retooling — in new regions or in new theoretical questions — requires continued attention by our membership.

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