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Letter from the outgoing editors
What We've Learned

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Our editorship of *APSA-CP*, which began in fall 2002, ends with this issue. Our second four-year contract has expired, and we are both eager to move on to other projects.

Nevertheless, we are grateful to the Section for having entrusted the editorship to us for such a long period. We hope we have provided a useful service to our readers. Serving as editors has definitely been beneficial to us personally, as well as to our department and university.

It has also been a valuable learning experience. Editing the newsletter has given us a privi-

leged vantage point from which to survey the discipline and the emerging scholarship of its members. In our effort to generate new and stimulating symposia, reviews, and features that explored topics of general interest that were not already well discussed by other publications, we often had to venture into unfamiliar territory. As a result, we learned, among other things, that the boundary between comparative politics and international relations has been erased to different degrees in different spheres: almost completely in political economy but not much at all in the study of institutions. We got acquainted with the burgeoning literature on civil wars and the hard-fought debates that roil it. We also introduced our readers, and ourselves, to emerging methods, from advanced statistical techniques, to identification of causal mechanisms, to automated content analysis.

> "Editing the newsletter has given us a privileged vantage point from which to survey the discipline and the emerging scholarship of its members."

As part of our effort to survey the subfield, we commissioned several of our students to investigate trends and tendencies in Ph.D. dissertations (Summer 2005) and

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comprehensive examination reading lists (Spring 2003). They uncovered some surprises. Who knew that there were more doctoral dissertations on China than on any other country, and more on East Asia or Latin America than on Western Europe? The paucity of dissertations employing a predominantly rational-choice approach (4 percent from 1985 to 2004) contradicted a commonly held perception among comparativists, although the percentage indeed rose after 2000. It was not a surprise that dissertations tend to be event-driven and increasingly quantitative, but the overall picture is one of a subfield that is methodologically pluralistic and regionally diverse. This conclusion was reinforced by the reading-list analysis, which reported very little consensus about what graduate students should be reading for their initial preparation in comparative politics: only nine books or articles -- or less than one percent of the total -- were assigned on at least half of all reading lists. Also striking was the extent to which these lists emphasize works on Western Europe or political economy. There appears to be a mismatch between what we train students to do (and/or what we consider to be exemplary scholarship) and the topics of their dissertations.

> "Who knew that there were more doctoral dissertations on China than on any other country, and more on East Asia or Latin America than on Western Europe? The paucity of dissertations employing a predominantly rational-choice approach (4 percent from 1985 to 2004) contradicted a commonly held perception among comparativists..."

A symposium on "Big Unanswered Questions" (Winter 2008) provided a snapshot of the research agendas of twenty leading scholars. We had hoped that some common themes would emerge, and one did, as thirty percent of those participating posed questions about the cognitive, subjective, cultural, or ideational dimension of politics. Perhaps these shared questions foreshadow greater attention to what goes on in political actors' heads rather than the political environment in which they are immersed. We had wanted to craft a symposium on politics and cognitive psychology, but it took us several years to figure out how to define the topic and the issues that needed to be addressed, so alien was it to our own areas of research expertise. The symposium that eventually came together ("Politics and the Brain," Winter 2010), offered new ways of thinking about the interaction between internal (cognitive, emotional, developmental, genetic) factors and the external ones that dominate most research in comparative politics.

Another multi-contributor symposium, "Concepts as a Hindrance to Understanding...and What to Do

about Them" (Summer 2009) exposed a shocking (to us, at least) absence of consensus about many of the concepts that are the foundation for our subfield — "state," "rationality," "democracy," and "moderate opposition" among them. We strongly doubt that we picked unrepresentative cranks to opine on this topic; rather, we believe that any reflective comparativist is uneasy about at least a few of our common concepts and that there remains considerable room for improvement. In the long run we expect that a near-consensus will evolve around the most useful ways to understand comparative political phenomena. The fact that this apparently has not happened yet suggested that comparative politics is still in an early phase of development as a subdiscipline.

One of the ways in which political

scientists crystallize certain definitions is by operationalizing them, when we produce data. An ongoing feature of *APSA-CP* has been the regular reporting and reviewing of datasets by a series of Notre Dame graduate students. When we began our tenure as newsletter editors, we worried that we might run out of new datasets to announce. However, this potential problem never materialized. Comparative politics no longer suffers from data scarcity, although there are certainly specific concepts that remain unmeasured. But as the dataset reviews have suggested, obtaining valid and reliable data on exactly the actors, times, and places we would like to study remains a serious challenge.

In the process of brainstorming about topics and recruiting authors, we have relied on a core editorial board consisting of Notre

Dame's comparative politics faculty, and on the rotating membership of the Executive Committee of the Organized Section on Comparative Politics, during the presidential tenures of Evelyne Huber, Peter Hall, Peter Gourevitch, Sidney Tarrow, and Susan Stokes. We have also corresponded with hundreds of comparativists in all the major departments, all over the world. We had previously known many of these people only through their publications, but now we are acquaintances or even friends. Perhaps the most unexpected reward of this experience has been the discovery that many of the people one would expect to be unapproachable due to their professional prominence, research productivity, or institutional appointments have often turned out to be among the most generous and gracious. We are deeply grateful to them all.

Symposium
Synergies Between Comparative Politics and Political Theory

The theme of this issue's symposium, our final one as editors of *APSA-CP*, is "Synergies between Comparative Politics and Political Theory." Like the two previous symposia which respectively examined the confluence of comparative politics and American politics (2004: Vol. 15: 1) and international relations (2005: Vol. 16:1), this symposium asks in which specific ways and to what degree comparative politics and political theory have been

enriched (or not) by the concepts, agendas, and methodological approaches of the other in recent years.

The immersion of comparativists in field research, their acquisition of deep regional knowledge, and their utilization of qualitative and quantitative data has sparked theoretical insights that have often prodded political theorists to probe experiences that transcend traditional modes of thinking. Political

theory, on the other hand, has historically posed normative questions that provide comparativists with a research agenda and provoked them to develop new concepts and frameworks for comparative analysis. For both camps, it could be argued, such borrowing has inspired scholars to ask more interesting questions, gain greater clarity in the way political phenomena are conceptualized, and achieve a more perspicacious understanding of many interesting

and timely subjects. This said, political theory appears to have intersected with comparative politics to a lesser degree than American politics and international relations. Has there been too little borrowing? Have political theory and comparative politics intersected too little? Has the current intellectual division of labor between the two subfields been necessary and functional?

We posed the above questions to our four symposium contributors, all of whom have considerable experience in traversing the intellectual and methodological boundaries between political theory and comparative politics. Their collective response, not surprisingly, is that the two subfields of political science can intellectually profit from and better advance their respective scholarly agendas through greater collaboration. More interesting are the different obstacles that our authors respectively perceive as impeding these ends and the intellectual fruits that they believe can be gained from greater subfield cross-fertilization.

Michaelle Browers begins the discussion by contending that an especially rich space for synergy between comparative politics and political theory lies in the examination of ideas which are situated within "their appropriate historical and empirical contexts." For political theorists this requires engaging practical problems and incorporating empirical historical and social scientific research into their inquiries. On the other hand, she advises, comparativists would profit from considering the wide range of works by contemporary political theorists with considerable experience and training in area studies and who possess

impressive foreign language facilities.

Jennifer Holmes argues that political theory has a special role in identifying "the conceptual inheritances that we may too readily accept without critique." From Holmes's perspective, comparativists tend to operate within the

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confines of what data are available, what "concepts have reliable indicators, and what types of interviews or fieldwork is feasible … [while] theorists invite fresh thinking about many core issues, assumptions, and concepts." She specifically cites democracy and globalization as especially productive subjects of collaboration between political theorists and comparativists.

Of the three essays, Banting and Kymlicka's offer the most critical assessment of the current divide between contemporary political theory and comparative politics. Echoing Browers, Banting and Kymlicka see political theorists as rarely attempting to subject their normative positions to the available social science evidence. According to the two authors, theorists "may offer a few anecdotes in support of the realistic nature of their positions, but serious assessment utilizing the methodologies developed by comparative politics is rare indeed." Conversely, they complain, comparativists too often seem preoccupied with second- rather than first-order questions.

How to bridge this divide? On the one side Banting and Kymlicka prescribe that the arguments of political theorists could be strengthened by submitting their empirical claims to more rigorous testing. On the other side, they recommend that by engaging more systematically with theorists comparativists can more clearly think "about which of the many questions we ask are especially compelling, and which of the myriad of relationships we investigate are critical." Although Banting and Kymlicka concede that a division of intellectual labor is a necessary and an enduring feature of modern intellectual life and, therefore, contemporary political science scholarship, they nevertheless conclude that more systematic collaboration between political theorists and comparativists can "yield considerable intellectual dividends." On this point we couldn't agree more.

Synergies Between Comparative Politics and Political Theory

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At the 2001 American Political Science Association (APSA) annual meeting, Rogers M. Smith noted a growing chorus of calls for more empirically engaged political theory beginning in the mid-1990s.[1] This new thinking in political theory might re-envision its relationship with comparative politics along the following lines. Political theorists would focus their efforts on engaging practical problems and incorporate empirical historical and social scientific research into their inquiries. Our colleagues in comparative politics, for their part, would draw upon the resources provided by political theory to craft arguments that could be analyzed empirically and to interpret empirical findings within a framework of values informed by theorists.

At the same time that Smith and others were debating empirical engagement, another trend was rising within the subfield of political theory that holds equally promising potential for conversations between comparative politics and political theory. In 1999, Fred Dallmayr presented a volume that sought to "help launch a new field of academic inquiry as well as general intellectual concerns: the field of comparative political theory."[2] Work by Dallmayr, Brooke Ackerly, Roxanne L. Euben, Hwa Yol Jung, Thomas Pantham, Bhikhu Parekh, and Anthony Parel – to mention just a few who have been at the forefront of the launch – sought to hammer out the normative and practical implications of this endeavor, to articulate an ethic for cross-cultural engagement, as well as to exemplify the fruits of such reflective cross-cultural and comparative theorizing that could inform those who wishes to better understand the societies with which we engage politically.

A recent assessment of the state of comparative political theory by Andrew March, as well as an equally penetrating response by Farah Godrej revealed at once the richness, variation, and unsettledness of this path of inquiry.[3] Despite their differences about what should be the aims and methods of the work undertaken by those theorists who tend to be placed under the subheading of "comparative political theory," they seem to agree that, in March's words, engagement of "texts and thinkers from outside the West has the capacity to change our present opinions about normative commitments, their scope and epistemic status, and the social and institutional conditions for their realization" (p. 544), as well as to, in Godrej's words, "bring[…] to light entirely new kinds of questions arising from new preoccupations and new

> "...one of the richest spaces for synergy between comparative politics and political theory is being carved out by new generation of political theorists who have emerged as a distinct stream within the considerable space opened up by comparative political theory."

frames of inquiry" (pp. 581-2).

It is my contention that one of the richest spaces for synergy between comparative politics and political theory is being carved out by new generation of political theorists who have emerged as a distinct stream within the considerable space opened up by comparative political theory. These political theorists are perhaps not unique to the subfield in seeking to situate the ideas they study in their appropriate historical and empirical contexts. All of these scholars I will discuss here have considerable experience and training in what might be considered "area studies" and impressive language skills. While all of these scholars are in political science departments and tend to identify political theory as their primary subfield, most are comfortable listing comparative politics as a second field and many teach courses in both subfields.[4]

One of the effects of expanding the "canon" of political theory seems to be an increase of young scholars with extensive foreign language, area studies and comparative politics training who are finding political theory a more hospitable home than in the past as they conceptualize their research

agendas, as well as an increase of young scholars from "non-western" backgrounds who are entering the subfield. Their scholarship should be of interest to those who seek to comprehend the particular tradition, context, or area studied; but their work holds further potential for uncovering contestations of values and norms that can affect both those situated within those traditions and those who seek to understand from without.

Among the very few political scientists who can speak authoritatively about Yemen is the political theorist Lisa Wedeen, whose recent book, *Peripheral Visions: Public, Power and Performance in Yemen* (University of Chicago, 2008), not only expands our understanding of Yemeni political practices but more broadly the complex and surprising ways in which national solidarities and identities can operate in weak states. So too, her earlier book, *Ambiguities of Domination* (University of Chicago, 1999), offers one of the field's most compelling analyses of the way in which in which the use of rhetoric and symbols operated as a disciplinary device in Syria under former president Hafiz al-Asad.

Comparativists with an interest in the role of religion in politics will find a rich store of works by comparative thinking and empirically informed political theorists. Any study of religious movements would do well to consider Roxanne Euben's writings on "fundamentalism," particularly her first book, *Enemy in the Mirror* (Princeton University Press, 1999), which interrogate the implications of using social scientific models of explanation for understanding political Islam. Andrew March's writings on Islamic political thought and legal theory, including his recent book, *Islam and Liberal Citizenship* (Oxford University Press, 2009), should inform scholarship that

> "Many of the strongest voices for and finest theorists within comparative political theory are more concerned with thinking comparatively in order to help open up the array of alternative visions of human possibilities offered up by first-order texts from non-western contexts."

examines contemporary debates about policies aimed at integrating Europe and North America's recent Muslim immigrants. Nader Hashemi's study of *Islam, Secularism, and Liberal Democracy: Toward a Democratic Theory for Muslim Societies* (Oxford University Press, 2009) should be essential reading for scholarship that engages questions of democratization in Muslim majority contexts.

My own analysis of the history of and recent debates over notions of democracy and civil society in Arab political thought was intended to move us beyond scholarship that claimed that lack of democracy among Arab states could be attributed to a lack of cognizance of democratic ideas and interest in international debates over democratization, as well as to point scholars toward the way in which these concepts have been contested and reconceptualized in this context.[5] My more recent work, *Political Ideology in the Arab World*, attempts to link up to recent comparative work that tests the idea that inclusion (par-

ticularly of Islamists) leads to moderation by analyzing the intellectual trends that both predate periods of inclusion and contribute toward cooperation across ideological divides.[6]

Most of the examples about which I can speak most authoritatively are in Arab and Islamic political thought – which may point to how far this political theorist has moved into an area specialization. However, this synergy between political theory and comparative politics extends well beyond the Middle East. Two examples that I am most familiar with will have to suffice. Juliet Hooker's book, *Race and the Politics of Solidarity* (Oxford University Press, 2009), contributes to our understanding of the experiences of American (North and Latin, with a particular enlightening chapter on Nicaraguan) movements that seek to remedy racialized oppression and enable just accommodation of cultural difference. In regard to Chinese political thought, the already well-established record of Leigh K. Jenco's careful attention to Chinese sources leaves little doubt that her work, *Making the Political* (Cambridge University Press, 2010), will likely prove important reading for China specialists and scholars of social and political movements.

The number of political theorists who focus so much of their effort on situating the ideas they engage and, thus, often look like area specialists may in fact be worrying to some theorists who prefer to think of their endeavor as comparative or who consider generating universal truths or identifying central cores common to all human beings regardless of geographical location to be political theory's primary responsibility.[7] Many of the strongest voices for and finest theorists within comparative political theory are more concerned with thinking comparatively in order to help open up the array of

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alternative visions of human possibilities offered up by first-order texts from non-western contexts. However, the growing number of theorists who have immersed themselves in an area in order to write on Arab, Islamic, Chinese, Confucian, Indian, Latin American or any other delineation of an area of political thought, need not be worrying to colleagues in comparative politics, who I suspect will be more inclined to judge these works by how well they inform and enrich comparative political science rather than how well they speak to debates in political theory.

Further, I think that we can expect this trend to continue, as these young scholars continue to find tenure-track jobs, locate publishers who like the broader appeal of their work, and find colleagues in comparative politics who read and engage this scholarship. In regard to the latter, the last annual American Political Science Association meeting is any indication, these political theorists finding comparativists willing to share panels on topics such as "Women, Authoritarianism and Conflict,"

"Religion and Politics in Asia," and "Islam, Civil Society and Democracy."

Notes

[1]Rogers M. Smith, "Reconnecting Political Theory to Empirical Inquiry, or, A Return to the Cave?," in *The Evolution of Political Knowledge: Theory and Inquiry in American Politics*, ed. Edward Mansfield and Richard Sisson (Ohio State University Press, 2004), pp. 60-88. Commentaries on Smith's essay in the same volume by William Galston and Jack Knight flesh out a bit more as to why this trend is desirable for political theory and why it is equally important that empirical research engage political theory, respectively.
[2]Fred Dallmayr, "Introduction: Toward Comparative Political Theory," in Fred Dallmayr, ed., *Border Crossings: Toward a Comparative Political Theory* (Lexington Books, 1999), p. 1. Dallmayr rightly notes recent concerted attempts to expand the global focus of political theory draws on a much longer history of comparative political theorizing and of theorists engaging non-western political

thought.
[3]See Andrew March, "What is Comparative Political Theory?", pp. 531-65, and Farah Godrej, "Response to 'What Is Comparative Political Theory?'," pp. 597-82, in *Review of Politics* 71 (2009).
[4]That political theorists often teach outside the subfield of political theory should come as no surprise to anyone who has examined the results of the 2008 national survey of political theorists. See Matthew J. Moore, "Political Theory Today: Results of a National Survey," *PS* 43:2 (April 2010), pp. 265-72.
[5]Michaelle Browers, *Democracy and Civil Society in Arab Political Thought: Transcultural Possibilities* (Syracuse University Press, 2006).
[6]Michaelle Browers, *Political Ideology in the Arab World: Accommodation and Transformation* (Cambridge University Press, 2009).
[7]Dallmayr cautions that "theorists need to steer a middle course between narrow area specialists and abstract generalists." "Beyond Monologue: For Comparative Political Theory," *Perspectives on Politics* 2:2 (June 2004), p. 249.

Interaction, Innovation and Insight: How Comparative Politics Can Benefit from an Expanded Interchange with Political Theory

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For many years I have been praising the interaction of scholars at the European Consortium for Political Research's workshops and attending as many as I can. Small groups of

scholars from different approaches and perspectives within political science meet for five days, discussing each others' papers for an extended period of time. What is markedly refreshing is that theorists and comparativists participate in the same workshop, working on the similar topics without differences of approach or concept dividing them. What emerges from these interactions is that participating scholars ask better questions,

identify otherwise hidden assumptions, see opportunities for conceptual clarity or innovation, and face tough questions on the scope of their comparisons.

This short essay is not meant to provide a complete characterization of the two subfields and the potential or actual relationships between them, but rather to highlight some of the ways that I have found an interaction

between the two subfields to be productive in my own research and teaching. The collaboration of political theory with comparative politics can provide not only analytic clarity, but can also help to identify alternate aims of inquiry and highlight the proper scope of comparison.

Often, comparativists operate within the confines of what data are available, what concepts have reliable indicators, and what types of interviews or fieldwork is feasible. Theorists invite fresh thinking about many core issues, assumptions, and concepts. Indeed, as Coppedge (1999) reminds us, "much of the political theory we find interesting concerns some of the messiest concepts – power, participation, legitimacy, identity, development, accountability, stability, and democracy." These "thick" and contested concepts are ones that particularly invite productive insights from political theorists. Although putting these concepts into use in comparative study may be more cumbersome or difficult to measure compared to using conventional concepts with readily available datasets, they have been useful in a number of areas.

Development is one such messy concept. The meaning of development and the aims of economic growth are contested. Is it growth for growth's sake (as indicated by GDP per capita) or is something else expected to happen, such as broad based improvement in the individual's ability to make meaningful choices about their lives? Amartya Sen (1985, 1999) and Martha Nussbaum (1992) have redefined development in terms of human capabilities and functionings, instead of economic growth as measured by GDP per capita. Instead, their focus is on opportunity and individual agency. Nussbaum conceptualizes development more in terms of basic human functions, capabilities and limits, and

ideas of a good life. Partially in response to Sen's criticisms, a broad measure of development, the Human Development Index, was created. According to its first human development report, "Technical considerations of the means to achieve human development … have obscured the fact that the primary objective of development is to benefit people" (UNDP 1990, 9).

Similarly, my own research (Holmes 2001) has challenged conventional

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understandings of the relationship between regime support and the concept of political legitimation. Most contemporary concepts of legitimacy (evaluative, socialization, or procedural) fit within Max Weber's (1946, 79) characterization of the legitimation of domination, which are either traditional, rooted in charisma or based on legal-rational foundations. For example Robert A. Dahl (1960, 46n), defined legitimacy "not in an ethical but in a psychological sense, i.e., a belief in the rightness of the decision or the process of decision making." This work has been broadly influential throughout political science, establish-

ing a conception of democracy based on political procedures and the political liberties necessary for those processes to work. Dahl's seven institutions of polyarchy remain foundational for the study of contemporary democracies: elected officials, free and fair elections, inclusive suffrage, the right to run for public office, freedom of expression, existence and availability of alternate information, associational autonomy.

However, circumstances, events, and public opinion in countries such as Peru, Uruguay, and Spain lead me to ask if a different concept could help to explain the popular support or lack thereof for regimes. Theorists working within an Aristotelian framework suggest an alternative approach based on purposes or ends instead of process. Specifically, Steven Salkever (1981, 492) states that "before it is possible to live well, it is necessary for us to live and to live together." Stability and integration are necessary to provide a foundation upon which citizens can hope to achieve virtue, although they shouldn't be confused with ultimate goals. In the end, a concept of purposes of the state seems more appropriate to understand when regimes maintain support and when citizens abandon a regime for another. This purpose-based framework is radically different than a process-based approach.

Political theorists also direct comparativists to fundamental questions concerning the scope of comparison, and ask how our methods and concepts may be historically or temporally embedded in certain contexts. Laitin (1998, 432) argues "that our questions change (and the way that we ask them changes) depending on the political milieu of the theorist is a crucial and continuing lesson taught in political theory. Political theory demonstrates that political patterns and polit-

ical values need to be historicized because if they are not, claims to generality are likely to prove false." Similarly, political theory can also help us identify the conceptual inheritances that we may too readily accept without critique. For example, Tilly (1984), in his essay on comparative and interdisciplinary work in the social sciences, argues that scholars need to free themselves from the burden of 19th century thinkers and the eight pernicious postulates that emerge from them. For example, he targets notions of the "society as a thing apart," block notions of social change, stage theories of development, an emphasis on differentiation "as a master principle of social change" (1984, 43), integration and control, and sharp dichotomies between order and disorder. He pleads with scholars to free themselves from the "nineteenth century incubus" that continues to influence social science research (1984, 2). Reading works that provide new concepts and different time and area points of comparison can help scholars meet that challenge.

Similarly, democracy and globalization have also been the topic of a productive collaboration between political theory and comparative politics. For example, scholars such as Kaufman-Osborn 2006 and Shapiro 1996 have emphasized that many of our categories and the disciplinary divides in political science are linked to the Cold War and may not travel well to contemporary times and challenges. Specifically, Kaufman-Osborn (2006, 70) doubts the usefulness of "a set of categories that privileges the nation-state as a political form" given the current post-Cold War reality and some of the contemporary challenges facing both scholars and individuals. In another example, David Held (2004) characterizes the modern state and state-based accountability as insufficient for our current global interac-

tions. Instead, he advocates a "global polity with multilevel citizenship as a cosmopolitan order" (Held 2004, 388).

Studies of forced migration, internal displacement and asylum also demonstrate a useful challenge to state-centric approaches to comparative politics. Recently, Hedman and Gibney (2008) coedited a collection of works on forced migration that included scholars at the intersection of comparative politics, political theory, and international relations motivated by a belief that a "focus on the 'political' can help shed further light on the dynamics and processes involved in the representation, contestation and control of forced migration, and, critically, on the powerful effects thereof for democratic governance and civil society writ large" (2008, 139). These topics challenge conceptions of territorial sovereignty and similarly invite insights from political theory. Kernerman (2008, 232) reminds us that "refugees have long been understood as figures of exception in the international order of nation-states – as Hannah Arendt, Liisa Malkki, Nevzat Soguk and Giorgio Agamben have demonstrated." Landau and Monson (2008, 315) state "the ability to define who has the right and ability to occupy territory and to access benefits of residence is central to standard, modernist conceptions of state sovereignty." They characterize the conventional understanding of sovereignty as being informed by assumptions of the unitary actor, dichotomous sovereignty, state-centered practices of sovereignty, and the constraint of state actors by the state's laws and principles. Similarly,

Bassel (2008) uses Althusser-inspired "models of citizenship" to understand how Somali women refugees have differing prospects in terms of integration, identity, and citizenship in two different contexts: Canadian multiculturalism and French republican assimilation.

The dialogue between political theory and comparative politics should be productive but concepts from theory cannot be automatically applied without reflection. Concepts, aim, and scope still need to be justified as appropriate and should help provide context and an understanding of empirical data. But that warning also applies to the concepts, aims, and scope that we have inherited and may unquestionably apply in our studies. In a study of the applicability of ancient political philosophy, Nichols (1979) counsels us to look at the particular societies and their principles to determine whether the application can be useful. Regardless, fresh thinking about messy concepts is always welcome. As Scott (1995, 37) warns, "if you see the world only through your instruments, then it is likely to be a world that is hard to broaden and that may very well be poverty stricken." Reading and responding to the issues raised by political theory can help comparativists shift the locus of the question, provide grist for conceptual innovation, and clarify the scope of comparison in terms of time and scope.

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> "The dialogue between political theory and comparative politics should be productive but concepts from theory cannot be automatically applied without reflection."

Diversity and Democracy in Political Theory and Comparative Politics

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The relationship between comparative politics and political theory has tended to fluctuate between benign indifference and casual cherry-picking. In our view research in both subfields can be enriched by closer and more systematic collaboration.

While political theory is of course primarily prescriptive not descriptive – it tells us what ought to be, not what is – most political theorists nonetheless insist that their prescriptions are compatible with well-established social scientific findings about the nature of human beings and of social dynamics. As Rousseau famously said, the goal of political theory is to "take men as they are, and laws as they might be," and in this sense political theory aims at what Rawls calls "realistic utopias," consistent with what we know about "men as they are." Unfortunately, political theorists rarely attempt to subject their normative positions to available social science evidence. They may offer a few anecdotes in support of the realistic nature of their positions, but serious assessment utilizing the methodologies developed by comparative politics is rare indeed.

Conversely, the work of comparativists

also suffers from their distance from political theory, although in a different way. The first creative step in conducting research is choosing which questions to ask. Which questions are most compelling? Which reflect second-order concerns, helpful perhaps in answering more foundational questions but less important in their own right? Too often, comparativists seem to focus their attention on second-order questions. A closer engagement with political theory can help us focus on the most important questions.

To illustrate the potential benefits of a closer collaboration, we draw on the field of ethnic politics. Both comparativists and theorists have engaged issues in this field. But interactions between the two have been episodic at best, and a more systematic approach holds promise for both. To make this case, we start by summarizing the central debates in the field, and the traditional nature of the relationship between political theory and comparative politics. We then explore the advantages of closer collaboration between the two subfields.

The Debates on Ethnic Diversity and Democracy

Until the late 1970s, many scholars of comparative politics and political theory implicitly assumed that modernization would lead to the dissolution of ethnic identities and the disappearance of ethnic politics. Increases in levels of education, mobility and communications were expected to erode ethnicity; and prior to the work of scholars such as Lijphart and others, existing forms of ethnic minority rights

(such as consociational power-sharing in Lebanon or, in another field, the special status of American Indian tribes) were seen as premodern anachronisms, unworthy of sustained empirical study or normative reflections. Both subfields operated (implicitly or explicitly) on the assumption that a "normal" (i.e. developed, modern) state was one in which a common nationhood was established and ethnicity was depoliticized.

In reality, ethnic politics has proven to be an enduring feature of modernity. There has been a dramatic revival of ethnic politics since the 1960s, whether in relation to "old" minorities (indigenous peoples, national minorities, African Americans) or "new" minorities (immigrants). And these groups typically mobilize not to contest modernity in the name of preserving premodern customs or traditions. Most often, they mobilize to create space in which they can participate in modernity in their own way, in light of their own customs and in some cases in their own language. In effect, they seek to claim modernity as their own.

Both comparative politics and political theory have sought to understand the persistence of ethnic politics, the challenges it raises, and the options available to states to respond to it. There is now a rich twenty-year-old literature in the comparative politics of ethnic relations, and in normative political theories of multiculturalism/minority rights.

What have the two fields learned from each other? So far, at least, we would say very little. The two literatures have developed more or less in isolation

from one other, and what little borrowing that has occurred has operated primarily to reinforce existing positions within the two subfields, rather than encourage new insights.

To oversimplify, we can see two main positions on ethnic politics within both comparative politics and political theory. The first position sees ethnic politics as regrettable and dangerous, and as something ideally to be overcome. It is acknowledged that minorities often have good reason to mobilize, because of the economic, political, cultural disadvantages they face within nationalizing societies defined by a hegemonic group -- disadvantages which are not removed simply by declaring difference-blind laws. These disadvantages may even require exceptional temporary measures such as affirmative action, transitional bilingualism or, in post-conflict situations, transitional power-sharing. But the long-term goal should be to create a genuinely inclusive non-discriminatory difference-blind society in which minority mobilization would not be necessary. Ethnic politics are always regrettable, according to this view, because they are prone to multiple pathologies: polarizing identities and exoticizing others in a way that inhibits solidarity and diminishes the space for democratic deliberation and negotiation; encouraging ethnic outbidding; rewarding unrepresentative rent-seeking, ethnic entrepreneurs; imposing constraining scripts on group members, particularly on their more vulnerable members; and so on. Wherever possible, therefore, the goal should be to discourage ethnic politics, and certainly not to institutionalize it. We can see this view both within comparative politics (e.g. Horowitz; Roeder; Brubaker; Sniderman) and in political theory (eg., Barry; Waldron; Laborde), and not surprisingly they tend to reciprocally cite one another.

As against this view, others in both comparative politics and political theory emphasize that creating space for ethnic politics is a way of opening up democracy and enabling effective minority participation, thus allowing for the ongoing renegotiation of public space and public discourse to better fit people's evolving identities. The goal of ethnic politics, in this view, is not a permanent settlement of past or present injustices that will henceforth remove the need for ethnic politics. Rather, democratic politics is always a site where people legitimately seek to publicly express and politically negotiate identities in an ongoing process. The public expression of these identities is itself a legitimate and valuable form of democratic citizenship, and contributes to building a broader culture of human rights, democratic freedom, solidarity, and tolerance. Again, this view is prevalent within both comparative politics (e.g., Brysk; Van Cott; Gurr) and political theory (e.g., Tully; Young; Kymlicka), and unsurprisingly

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scholars from each subfield reciprocally cite one another.

As a result, learning between comparative politics and political theory has tended to be self-reinforcing rather than self-reflexive. Political theorists selectively cite those comparative politics studies that confirm their pre-established arguments, and vice-versa. The question is whether we can do better.

Doing Better

On one side, the arguments of political theorists would be strengthened by more rigorous tests of their empirical claims. To be sure, testing theoretical ideas is not a simple task. Political theory thrives on highly refined concepts, and the subtle distinctions between them can be difficult to operationalize in the messy world of empirical research. For example, the argument by some theorists that political claims based on ethnic identity are inconsistent with the norms of democratic citizenship seems to be contradicted by comparative political studies which show that claims for indigenous rights in Latin America have helped strengthen the norms and practices of democratic citizenship. But the theorist may respond that these studies didn't properly distinguish "identity"-based arguments from other arguments for indigenous rights, and/or didn't take into account the full range of norms of democratic citizenship. Moreover, as David Miller has recently pointed out, political theories seldom stand or fall on the basis of one particular empirical claim. The theory can usually be modified to take account of counter-examples. Still, many of the claims made by political theorists can and should be empirically scrutinized, and theories resting on empirically precarious foundations become increasingly less plausible, or so attenuated as to be uninteresting,

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eventually fading into insignificance.

On the other side, the work of comparativists will hardly be revolutionized by working more closely with political theorists. After all, in our own way, we are already asking many of the questions theorists ask. But engaging more systematically with theorists can help us think more clearly about which of the many questions we ask are especially compelling, and which of the myriad of relationships we investigate are critical. Political theorists can help us think about what matters most and why. They can help us answer that always irritating question: So what?

Consider the intense debates about the impact of increasing ethnic diversity on contemporary society and politics. Comparativists have deployed their full range of techniques to analyze the implications of ethnic diversity for such phenomenon as "social integration" or "social cohesion." But these macro-indicators themselves are contested, not only because of their inherent diffuseness but also because we are uncertain whether high levels of social cohesion and stability are inherently desirable conditions. In some cases, social cohesion may come at the cost of the suppression of vulnerable groups. Partly in response to these concerns, analysts have focused on more specific social indicators, such as interpersonal trust, trust in government, feelings of national identity, participation in civil society, support for redistribution, and so on. But how are we to sort through this complex array of attitudes and relationships? Which ones matter most? And which ones are important primarily because they are conducive to other outcomes we value more? Phrased in the terms of empirical analysis, which should be our dependent variables and which should we see as mediating variables?

Engaging political theory can help

here. Political theorists specialize in thinking about social states that we should value for compelling normative reasons. Thinking in these terms can help comparativists identify a hierarchy of relationships worth analyzing. For example, in our collaborative work, we argue that research in this field should focus primarily on the following three core dimensions of solidarity:

Civic Solidarity: characterized by mutual tolerance; an absence of prejudice; a commitment to living together in peace, free from inter-communal violence; acceptance of people of diverse ethnicities, languages and religions as legitimate members of the community, as belonging, as part of "us"; and an openness to newcomers from diverse parts of the world.

Democratic Solidarity: characterized by support for basic human rights and equalities, such as the equality of men and women; support for democratic norms and processes, including the need to advanced reasoned positions in public debates, equal participation of citizens from all backgrounds, tolerance for the political expression of diverse cultural views consistent with basic rights and equalities, and acceptance of compromises among legitimate contending interests.

Redistributive Solidarity: characterized by support for redistribution towards the poor and vulnerable groups; support for the full access of people of all backgrounds, including reasonable access for newcomers, to core social programs; support for programs that recognize and accommodate the distinctive needs and identities of different ethnocultural groups.

We suggest these three dimensions because, in our view, they are inherently valuable features of a society, and indeed are essential if a society is to be minimally decent and just. And

yet none can be taken for granted. All three may require individuals to act against their initial inclinations and self-interest, or at least to exercise self-restraint in the pursuit of those interests and beliefs. All three forms of solidarity, therefore, must continually be nurtured.

By contrast, we suggest other potential variables are best seen as mediating variables. In our view, dimensions such as trust, identity and civic participation are worth studying because they may be essential to ensuring a decent and just society. It may be impossible to achieve civic tolerance, commitment to a pluralistic democracy, and redistribution without the right kinds of trust, identities and participation. For example, interpersonal trust and trust in government may contribute to tolerance, effective democratic governance, or support for redistribution; and a shared sense of national identity may increase social inclusiveness, enhance democratic governance or reinforce support for transfers to the poor.

But these relations are, we believe, conditional and contingent. Societies that exhibit high levels of trust, national pride or civic participation may not in fact be particularly tolerant of minorities, or particularly solicitous of the poor, or even particularly democratic. To take one obvious example, the United States is known for its high levels of national identification and pride, yet this does not translate into support for redistributive solidarity. Conversely, France is known for its low levels of trust, yet this has not prevented the building of and high political commitment to a robust welfare state.

Our goal should be to clarify these linkages, and to determine the conditions under which trust, identity and participation support decent and just societies and when they do not. For

this reason, we need to think more clearly in terms of mediating variables and foundational values.

Much work remains to be done here, but we believe that distinguishing mediating variables and foundational values in this way will help to dispel some of the undue pessimism that has arisen about the impact of ethnic diversity. For example, we would argue that much of the debate gener-ated by Robert Putnam's influential study of the impact of ethnic diversity has been misplaced, in part because in a number of cases both the sup-porters and critics of his argument have treated mediating variables as if they were inherent values.

Concluding Reflections

As with ethnic politics, a division of labor is an enduring feature of moder-nity, including modern intellectual life. The era of the renaissance person, capable of leaping over entire disci-plines in a single bound, has passed. There is no need for political theorists to become sophisticated methodolo-gists or for comparativists to become theorists in disguise. But closer and more systematic collaboration between the two fields clearly can yield considerable intellectual divi-dends.

Addendum: Concepts That Hinder Understanding...

Editors' Note: During the Summer 2009 issue, we published a symposium featuring short articles focused on exposing con-cepts hindering progress in comparative politics. Errors on our part as well as space constraints meant that we could not include all of the concept articles that we received. Between the last issue and this issue we are publishing the contributions that were not printed in the original Summer 2009 symposium.

Market Reform

"Market reform" is an intuitively appealing concept referring to the widespread phenomenon of economic liberalization sweeping a majority of countries. However, this concept is misleading and creates both concep-tual and empirical problems. Political scientists have done valuable work in differentiating and fine-tuning con-cepts such as "democracy" and "the state" but not enough conceptual attention has been directed to the concept of "market reforms."

First, and most obviously, it is an expansive concept with broad bound-aries. The range of phenomena that it refers is wide and fuzzy referring to a whole range of policies ranging from privatization to a conservative mone-tary policy to private sector develop-ment. What can we exclude from its purview? Almost nothing. Moreover, the mere concept does not allow us to measure the extent or scope of mar-ket reforms. It is not clear what boundary threshold would qualify as market reforms: 30% or 50% privati-

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zation? Is the strength or effective-ness of reforms part of the concept at all? Secondly, and more importantly, the common notion of market reforms obscures a wide variation in the design and implementation of market reforms. Reforms ranging from changes in the former Soviet Union to those in United States can be includ-ed in the phrase but their import and design is very different in the two con-texts. Third, it conjures a vision of markets that are separate from socie-ty or the state and able to exist sepa-rable from them. In reality even the freest of markets are embedded in state action and public rules. The myth of a "free market" only exists as an ideal type in neoclassical models. Concepts in political analysis, by their necessary abstractness, cannot repre-sent reality perfectly. Yet, the notion of market reforms, by its bluntness, fuzzy character and inability to cap-ture even imperfectly the interesting variation in the world of economic lib-eralization, fails to be of much service to comparative politics.

So, what is to be done? The concept is helpful only insofar as it points to a movement away from something, for example, a state dominated economy. Scholars must use it in that limited sense. For purposes of empirical or comparative analysis, it may be better to use concepts that are more mean-ingful and substantive such as gover-nance reform, privatization, deregula-tion, fiscal stabilization, trade liberal-ization, or competition oriented poli-cies. The term "market reform" is a general concept that can be used to indicate a sea change in the rules of an economy, but should not be con-strued to indicate any particular goal or outcome or uniformity of design.

Book Review

The Enduring Theoretical Relevance of Rabushka and Shepsle's *Politics in Plural Societies*

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Recent work in comparative politics has focused attention on the strategic role of ethnicity in political conflict (Lyall, 2010; Cederman et al. 2010; Birnir 2007; Wilkinson, 2004). Therefore, the re-issue (Rabushka and Shepsle 2008, Longman) of one of the first books in comparative politics to "take a self-consciously rational approach" (Fearon, 2009), and certainly one of the first books on ethnic conflict to do so, is appropriate and welcome. Similarly, a re-examination of the theory put forth in Rabushka and Shepsle's *Politics in Plural Societies* in light of more recent contributions on the topic is overdue.

Necessary but not Sufficient Conditions for the "Plural Society"

The book is cited by hundreds of authors and has had a substantial influence on the field. Perhaps the most influential idea, as summarized by Grofman and Stockwell, is that "in Plural Societies ethnic salience and ethnic outbidding undermine multi-ethnic cooperation and inevitably lead to non-democratic, ethnically exclusive states" (2003: 106). A widespread misunderstanding, however, is that "*Politics in Plural Societies* … [is also] based

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on the proposition that ethnic identity is inevitably and effectively politicized" (Lustick, 1979: fn 16). In fact, Rabushka and Shepsle (R&S) propose an explanation for ethnically based conflict only when preferences on ethnic issues are mobilized. Cultural diversity, therefore, is a necessary but not sufficient condition for the contentious politics of a plural society.

R&S do not explain how ethnic preferences in plural societies become intense. They do suggest that ethnic mobilization likely rests on the history of value formation and political entrepreneurship in an environment that allows for mobilization on the basis of ethnic cleavage. Pre- and post-independence politics offer one exmple of institutional change that

transforms the political game from one of extraction to one where political entrepreneurs capitalize on ethnic divisions to the detriment of societal cohesion. If the above sounds close to constructivist ideas about instrumental politicization of ethnicity (Chandra 2004; Posner, 2005) it is worth adding that R&S would argue that "primordial" identities "provide a gestalt that defines the available political alternatives" (66). The principal point on which the authors differed from subsequent literature, therefore, is not the instrumental choice of one possible identity over another for political purposes but the level of determinism of the underlying identity categories.

Once ethnic cleavages are mobilized, R&S apply utility theory's insights about preferences and risk acceptance to explain ethnic conflict escalation. In sum, mobilized "incompatible" (20) ethnic preferences create a zero-sum environment where small gains by one ethnic group translate into substantial losses for any other ethnic group. Furthermore, the intensity of mobilized preferences pushes the ethnic member to "go for broke" (73) or be a risk taker in that she prefers an uncertain lottery for her preferred outcome to the certainty of her second preferred outcome – compromise. This preference structure makes political compromise between ethnic groups very unlikely if not impossible.

The obvious criticism at this point

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would be one of tautology – ethnic conflict appears where ethnic lines have hardened. In light of recent thinking, this criticism would, however, sell short the contributions of the book. Currently, conflict scholars (Cederman et al, 2010; McAdam et al. 2001; Kalyvas et al. 2008; Wilkinson, 2004; Birnir, 2007) and others examining the effects of ethnicity on a range of outcomes (Posner, 2004; Birnir and Waguespack forthcoming, Chandra, 2009) are particularly interested in the mechanisms by which ethnicity influences outcomes. The admirable clarity of R&S's theory allows for tweaking the proposed mechanism to bring the theory into line with recent advances, as explained below.

Utility Theory: Ethnic Groups as Risk Takers or Risk Averse

For example, since R&S wrote *Politics in Plural Societies*, scholars have noted that "peaceful and even cooperative relations between ethnic groups are far more common than is

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large scale violence" (Fearon and Laitin, 1996). Furthermore, many ethnic groups live together under democratic conditions and cooperate peacefully despite being politically mobilized (Birnir, 2007). A revised mechanism should, therefore, explain why in most cases the utility of inter-ethnic collaboration outweighs the utility of ethnic conflict in plural (diverse and mobilized) societies. One possibility is that members of ethnic groups are "risk averse" rather than the "risk takers" postulated by R&S. Indeed, surveying the literature on risk, Lichbach suggests that unless individuals consider themselves pivotal, "dissidents are on the average more accepting of risk than is the general population." He also finds that individual risk acceptance increases with wealth, independence and lack of success (1998: 97). Therefore, assuming wealth, independence and lack of success are not concentrated within a particular ethnic group it stands to reason that members of the group are, on average, as risk neutral or risk averse as the general population. In turn, if members of ethnic groups are risk averse or even risk neutral, and "as long as both sides suffer some costs for fighting, then [conflict] is always inefficient ex post – both sides would have been better off if they could have achieved the same final resolution without suffering the costs" (Fearon, 1995: 383). If this is true then we would expect ethnic bargaining and compromise to be the norm and ethnic conflict an anomaly.

One important implication of this simple modification concerns the effects of outbidding. According to R&S the only alternative preferred

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by a member of the ethnic group to the lottery for her preferred position is an alternative that increases the probability of obtaining the ethnic member´s preferred position. If members of ethnic groups are risk takers their leaders likely take radical positions early, and following Fearon's bargaining logic, narrow the bargaining space that otherwise might exist between the two groups (1995, 387). Political entrepreneurs wishing to attract members of the ethnic group must then outbid the hardliners already holding the reins, with predictably extreme results and concomitant conflict. However, in a world of risk neutral or risk averse ethnic group members a bargaining space is more likely opened between groups by ethnic leaders, and outbidders may be able to outbid without exiting that bargaining space. Even if outbidders do exit the bargaining space their policies will likely be less radical than the policies of those trying to outbid hardliners. Furthermore, outbidders exiting the bargaining space will probably not be supported by a risk neutral or

risk averse ethnic constituency. Once we drop the assumption that ethnic publics are risk takers, outbidding is no longer a necessary corollary of ethnic politics, suggesting we need to improve the theory explaining outbidding.

Alternative Explanations for Ethnic Conflict?

Assuming ethnic group risk neutrality or aversion allows us to focus on the reasons for anomalous ethnic conflict. R&S propose issue indivisibility. However, "the issues over which states bargain typically are complex and multidimensional; [and] side payments of linkages with other issues typically are possible" (Fearon 1995, 382). While Fearon suggests issue indivisibility mostly occurs in the domestic arena, most comparativists would suggest domestic politics are subject to the same type of bargaining and payment complexity that exists in international politics. Minority separatist demands may be an exception, but the minority may also accept autonomy if threatened with a costly conflict.

Pursuing this line of thinking, commitment problems constitute a more likely source of conflict (Fearon, 1995: 401). R&S refer critically to Lijphart's work on consociationalism, but the subject of institutional solutions to the inter-ethnic comitment problem remains one of the more fruitful lines in the study of ethnic politics. In addition to Lijphart's work, the best known example of institutionally engineered balanced competition is Horowitz's (1985, 1990) suggestion to gerrymander majoritarian districts in such a way

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that election in a district requires a politician to formulate balanced appeals to all ethnic groups in that district. In contrast to Lijphart's consociational solution, which is criticized for making all politics about ethnicity, Horowitz' solution is thought to take ethnicity off the political table (Reilly, 2001; Wilkinson, 2004). This institutional debate continues as a fruitful venue for inspired scholarship with the latest contribution suggesting that the time has come to reconsider the institutional effects of ethnic voting in proportional vs. plurality systems (Huber, n.d.)

A third possible reason for ethnic conflict, drawing on the bargaining literature, is the incentive to misrepresent ethnic intensity and resolve in an effort to improve the group's bargaining position. Unfortunately, however, the bargaining position of the group is not improved unless the

resolve is credible (Fearon, 1995: 396). In ethnic politics, demonstrated credibility of resolve might take the form of organized minority protests and, on the part of the ethnic majority, severe repression of that protest. Since R&S, great theoretical strides have been made in explaining such instrumental elite mobilization (for an overview see Varshney, 2007). Importantly, one of the insights of this later literature concerns the limits of instrumentalism. Thus, "ethnic violence can [also] spiral because of political contestation over group boundaries that are not the result of elite manipulation" (Fearon and Laitin, 2000: 872). In short, intended and unintended resolve spirals might constitute an important part of the story.

In sum, when first published this book was pathbreaking in both substance and form. The enduring contribution remains what initially distinguished this outstanding work: the authors propose a clear, innovative theory that produces falsifiable hypotheses. Because of this clarity and as our knowledge advances, scholars may continue to fruitfully engage this work in multiple ways, with only a fraction of the possibilities suggested above. Thus, *Politics in Plural Societies* remains a classic read for anyone interested in ethnic politics.

Note

[1]According to Google Scholar.

Book Review
Politics in Plural Societies: A Reconsideration

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When Alvin Rabushka and Kenneth A. Shepsle's *Politics in Plural Societies: A Theory of Democratic Instability* appeared in 1972, it offered one of the first systematic efforts to theorize about ethnic conflict. It was also, as noted by James Fearon in a new foreword, one of the very first examples of rational choice analysis in comparative politics. Recently republished in a new edition, it is worth reflecting back on this seminal work. What does it still have to tell us almost forty years after it was written? I will argue that its continuing relevance is in the theoretical model it offers, and more generally in the insights it still has to offer to rational choice analysis. The empirical analysis in the book, in contrast, does not stand the test of time as well.

The central lesson the book has for the field today is its treatment of preferences. Rabushka and Shepsle (R&S) begin with an obvious fact that most rational choice theorists tend to downplay: people's preferences vary. Furthermore, R&S note, preferences vary not only in terms of rankings of preferred outcomes; they also vary in terms of intensity, and in terms of the salience of the issue (R&S 2009 [1972], pp. 43-61). Each of these observations is vastly significant for their analysis, and for

ours.

R&S's first theoretical premise is that people's preferences vary depending on their cultural values: "the erection of a Chinese temple constitutes a 'public bad' for Muslims" (p. 10), for example. Note their recognition that the things people want are not exclusively tangible: the construction of a Chinese temple is a "public bad" for Muslims not just because of the opportunity costs, but because Chinese beliefs are normatively offensive to Muslims. While this insight is compatible with a broad understanding of rational decision-making, it is rarely considered in recent modeling efforts. R&S show how to do so.

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Even more significant, R&S's treatment of the intensity of preferences has enormous potential for improving the practice of formal modeling. They suggest measuring intensity of preferences in terms of a lottery (pp. 49-52). They ask: if a person has a preference ordering A>B>C, and has a choice between getting B (her second choice) for sure, or entering a lottery in which there is some probability of getting A (her first choice), and some probability of getting C (her worst case), which will she choose? The answer depends, of course, on the probabilities. If the person prefers even a small chance of getting A—even with a large probability of getting C—over the certainty of getting B, then R&S say that she has an intense preference for A over B.

Factoring intensity of preference, defined this way, into their analysis, R&S offer a neat explanation of why political moderates often fail. Suppose one politician promises group A to give them *their* maximal demands, while simultaneously promising group B their maximal (and competing) demands. Assume the opposing politician forthrightly suggests a compromise. If voters' preferences are intense, R&S (pp. 53-55) point out, all voters should support the first politician, who offers them some probability of getting their first choice outcome (depending on which side he is "really" on). This politician can only be defeated, in R&S's logic, by an authentic advocate of the larger

side's maximal position, yielding a highly polarized political system.

This understanding is far more sophisticated and plausible than the more recently-preferred approach to ethnic conflict, and especially civil war, developed by Fearon (1995, 1998) and centered on information failures and commitment problems. Fearon points out that since war is highly costly, it should always be avoidable if the parties know *ex ante* who would win. Both sides should prefer a bargain granting the future winner's demands but avoiding the costs of war. From this perspective all wars (including ethnic wars) are the result either of an information failure, a commitment problem, or both.

The trouble with this formulation is that, as every theorist of war since Sun Tzu has noted, the outcome of war is never fully knowable *ex ante*, even if all knowable information is shared. The only way rational actors can think about war, therefore, is as a risky lottery. And as R&S's analysis demonstrates, actors in this situation can rationally choose war over compromise: if their preferences are intensely held, both sides may prefer even a small probability of victory in war—and gaining their most-pre-

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ferred outcome—over even a certain compromise. Neither information failure nor commitment problem is necessary. In cases of ethnic rivalry short of war, the R&S view has an even greater advantage over the Fearon model, as the existence of a functioning state tends to mitigate the commitment problem. In such cases, only the intensity of divergent preferences can explain the degree of ethnic contention.

At the end of the theoretical section of the book, R&S (p. 92) conclude that their "paradigm . . . is not complete for two reasons . . . [it lacks] 1. a theory of political entrepreneurship, and 2. a formal treatment of preference formation." As a potential research agenda, this statement represents not so much a challenge scholars have failed to take up, as one that most political scientists, regardless of theoretical approach, have run screaming away from. This is the second great insight of the book: that political scientists need to theorize these fundamental processes, and that formal modelers should include insights from these examinations in their models, instead of relying on theoretically useful but empirically unsupported assumptions.

Ironically, however, R&S use the

wrong empirical basis for their own theorizing: they start from the primordialist concept of ethnicity and the "plural society" model of ethnic conflict that are now almost wholly discredited. The whole notion of primordialism is, indeed, based in large part on a misunderstanding. Clifford Geertz, who is commonly cited by political scientists as the founding father of primordialist theory, was in fact nothing of the sort. Anthropologists see Geertz as a founder of the approach of *symbolic* anthropology, emphasizing the importance of symbols, meanings and interpretations in defining group identity: "culture," he notes, "is inevitably involved in such matters" (Geertz 1963, p. 109); group identities are *not* permanently fixed. R&S (p. 8) are probably among the first to mischaracterize Geertz on this issue: quoting his phrase "primordial attachments" out of context, they make him seem to be suggesting that identities are fixed.

There is now an interdisciplinary consensus that ethnic attachments are socially constructed, a point demonstrated early by Crawford Young (1965; 1976). The contemporary debate boils down to how malleable these social constructions are. Fortunately for R&S, their starting point remains tenable as long as these identities are relatively "sticky," changing only rarely. R&S can then bracket the notion of where ethnicity comes from, assuming for their purposes that such identities are at least usually stable parts of the social context in which politicians operate.

More problematic is R&S's reliance on the "plural society" school of

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thought, led by J.S. Furnivall and M. G. Smith (1965), which essentially assumes R&S's conclusion: that ethnically plural democracies are inherently politically unstable. As examples from Catalonia to Quebec show, this is not necessarily so. R&S's error is illustrated in the title of Saul Newman's (1996): *Ethnoregional Conflict in Democracies: Mostly Ballots, Rarely Bullets*, and demonstrated more narrowly in umpteen recent statistical studies (the seminal piece being Fearon and Laitin 2003) demonstrating that there is no linear relationship between ethnolinguistic fractionalization and civil war.

Part of the problem in R&S's treatment is their tautological definition of "plural society": "a society is plural if it is cultural diverse *and* if its cultural sections are organized into cohesive political sections" (p. 21). R&S's

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conclusion of inevitable ethnic tension therefore simply follows from their definition. If a polity is not only ethnically diverse, but the ethnic differences are political salient enough to lead to political organizations being ethnically based, then there will be lots of ethnic contention in politics.

The empirical analysis in the book's second half reflects this deterministic logic. R&S's discussion of South Africa (pp. 158-169), for example, usefully explains how the development of Apartheid can be understood in terms of their theory, but the result is that their conclusion rules out the possibility of change *a priori*: "The politics of extremism, as the theme of apartheid depicts, seems to preclude the viability of moderation on the racial issue by White politicians who seek electoral victory" (p. 167). It is, of course, understandable that in 1972 the authors would have seen the apartheid system as unassailable. But they globalize this assessment, leading to the stark conclusion: "Is the resolution of intense but conflicting preferences in the plural society manageable in a democratic framework? We think not" (p. 217).

This conclusion is insupportable. The ethnic politics of Quebec, Scotland, Wales, Brittany, Catalonia or even South Africa cannot reasonably be considered "unmanageable" or "unstable." Future analysts could save R&S's model by ditching its determinism and treating the intensity of preferences on ethnic issues as a measurable independent variable. Subsequent analysis would probably yield useful insights about how various intensities of prefer-

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ences yield different political patterns. But as written, R&S's theory simply assumes its pessimistic conclusion.

The reissuing of *Politics in Plural Societies* offers a useful occasion to reflect on how formal modeling has developed and on paths it might take. It reminds the broader field that more needs to be done to study political preferences and political entrepreneurship, and to take variation in preferences seriously. Its empirical analysis, however, is badly in need of rescue.

Complete citations for this issue are online at http://www.nd.edu/~apsacp/backissues.html.

Review: Constituency-Level Elections and Constituency-Level Elections Archive Datasets

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Introduction

Scholars of party systems and electoral politics will graciously welcome two new constituency-level electoral datasets, the Constituency-Level Elections (CLE) dataset by Brancati and the Constituency-Level Elections Archive (CLEA) dataset by Kollman, Hicken, Caramani, & Backer. Despite the fact that historical district-level data is a highly sought-after resource, the datasets by Brancati and Kollman et al. are among the first of their kind. (The Lijphart Elections Archive at UCSD contains 350 constituency-level elections but has not been updated since 2003.) While there is a high degree of overlap between the two datasets, scholars should take note of some of the important differences that are highlighted below. Depending on the scope of their research, scholars may find one of the datasets better suited to their interests than the other. Given the overlap between the two datasets, scholars may also find it useful to combine resources from the two data sets.

Brancati's Constituency-Level Elections (CLE) Dataset[1]

When completed, Dr. Brancati's constituency-level dataset will include sub-national data on voting totals and seat distributions for 60 countries. Although there are numerous datasets on electoral outcomes, the CLE dataset is the most comprehensive in terms of constituency-level election data for both upper and lower chambers of national legislatures. The dataset includes a fair amount of data on sub-national elections as well.[2] Whereas existing datasets have tended to focus on nationally prominent parties by imposing thresholds based on the number of votes received or seats won, the CLE dataset places no restrictions on recording vote totals. Where scholars are interested in variance in electoral politics on a sub-national level or on the role of minor political parties, they will likely want to look into Brancati's CLE dataset, especially where data exists on sub-national elections. Since the dataset applies no thresholds to the number of votes received, data on minor political parties is included in the CLE dataset where it might not be in other datasets that focus on nationally prominent political parties.

Conceptually, the data encoded in the CLE dataset is simple. For each district-level electorate, information is provided on the number of votes cast as well as the number of seats (if any) won by each party that received at least one vote. There are only two main variables in the dataset: number of votes cast and number of seats won. However, users should be warned that the dataset becomes quite large in countries with numerous districts and many political parties (which is the case for quite a few of the countries). For each party the data is coded as "s" and "v" corresponding to the number of seats and the number of votes. While this information is unfortunately not listed in the codebooks, it is easily discernable from a cursory glance at the data. The type of legislative seat under contention is distinguished on the national level by a 1 for the lower house and a 2 for the upper house and on the sub-national level by a 3 for the lower house and a 4 for the upper house. Each country dataset is accompanied by its own codebook, which include notes on significant details relating to the data.

The organization of the dataset allows the data to be relatively flexible to the needs of the user. Although some scholars may find it more efficient to use datasets that focus on nationally prominent political parties, the dataset is nonetheless easily adjusted. Variables can be collapsed into one another and as Brancati suggests the designation of parties as p1, p2, p3, etc. makes it easy to stack data from different countries on top of each other. Combining CLE data with other data sets may be a bit more challenging. Brancati advises that users review the datasets to be certain that a comparable level of information is provided in both datasets. However, this should not be a major concern with the CLE dataset since the district-level observations make it easy to collapse the data to fit less detailed datasets.

Currently only 44 of the 60 countries are available on the CLE website. When the dataset is completed it will include 60 countries with observations extending from 1944 to 2007. However, no country case includes data for the full extent of the dataset's scope. In fact, only 12 countries include data over the course of 50 years, while 22 countries include data for 10 years or less. These figures are by no means surprising when considering that the dataset only includes democratic elections. Nonetheless,

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some scholars may be surprised to find that a country of interest may not be included or that only one or two election cycles are included in the dataset. For instance, the United Kingdom, Chile, and South Korea have not yet been included. Currently, data for only 50 countries have been scheduled for release and it is unclear when data on the remaining 10 countries will be released.

There are, however, some major shortcomings of Brancati's CLE dataset, particularly in relation to the CLEA dataset by Kollman and his colleagues. Scholars interested in constituency-level analysis may desire more dimensions of data on the constituencies' electoral behavior, such as the level of turnout, size of the electorate, or district magnitude. While this data is included in the CLEA dataset, it is not available in Brancati's CLE dataset. Another significant shortcoming of the CLE dataset is its lack of information about the electoral system and relevant political institutions of each country. Depending on the research, data from districts with proportional representation may not be

fully comparable to data from a plurality voting system. Since the CLEA dataset accounts for both of these shortcomings in the CLE dataset, many scholars will prefer to use the dataset constructed by Kollman and his colleagues. However, if scholars are interested in particular cases they may find that Brancati's dataset is better suited to their needs. Likewise, where scholars are interested in upper-chamber elections or sub-national elections, the CLE dataset has compiled resources that are unavailable in the CLEA dataset or elsewhere.

Kollman, Hicken, Caramani, and Backer's Constituency-Level Elections Archive (CLEA)[3]

The CLEA dataset currently contains constituency-level data on lower chamber elections in 49 countries worldwide with data extending across the contemporary history of democratic elections. The purpose of the CLEA dataset is basically the same as Brancati's CLE dataset except that it restricted to lower chamber elections. Since comparative studies tend to focus primarily on lower chamber elections this will not present a problem for most scholars. Furthermore, the loss of data on upper chamber and sub-national elections is compensated for by a wealth of data on constituency characteristics, electoral systems and political institutions. Excluding research that is either interested in particular cases not present in the CLEA dataset or in upper chamber and sub-national elections, most scholars will prefer to use the CLEA over the CLE dataset.

Like Brancati's dataset, the CLEA provides constituent-level data on the number of votes cast and seats awarded. However, the CLEA dataset provides additional variables that offer greater detail concerning constituency

behavior and electoral results. Relating to constituency behavior, the CLEA datasets provides more detail than simply the number of votes cast. The dataset contains a variable for the number of eligible voters within a constituency, which allows for the calculation of turnout. The CLEA also distinguishes between first and second round elections where applicable and provides relevant variables for each round. In addition to the "votes cast" and "turnout" variables, the data set includes variables for the number of valid and invalid (or spoiled) votes.

Beyond variables relating to constituency behavior, the dataset also contains variables that aid in evaluating electoral results. Like Brancati's dataset, the CLEA contains a variable for the number of seats won. However, since the significance of the relationship of votes cast to the number of seats won is difficult to interpret without details concerning the electoral system, the CLEA dataset provides a number of variables that enable users to parse out the relationship between votes cast and seats won. For each constituency, the dataset includes a variable for the district magnitude, as well as the number of votes cast for each candidate, the share of votes cast for each candidate, the number of votes for each party, and the share of votes for each party. The candidate and party variables are separated into first and second round elections where applicable. In rare cases where voters choose electors and not candidates or parties, the dataset also contains a variable for the number of electors and the number of electors won by parties. Whereas the CLE dataset provides only variables for the number of votes cast and seats won per party, the CLEA dataset provides a number of variables that allow users to adjust the data according to the structure of different electoral systems.

The CLEA codebook goes above and beyond the expectations for what is considered user friendly. The codebook not only provides a clear and explicit outline of each of the variables, but it also provides a wealth of information on the electoral systems of each of the country cases. For each country the codebook includes an overview of how and when the country achieved independence, the type of political institutions and how they might have changed since independence, as well as changes to the electoral system by year. In addition, the codebook includes Polity scores for each country-year. Citations are included for the sources of both the electoral history and electoral data. The CLEA codebook is an impressive resource in comparison to the CLE codebook, which is restricted to a list of party names for each country and an outline of significant details relating to the data.

The CLEA dataset should also be considered in relation to the number of cases available for analysis in comparison to the CLEA dataset. The datasets currently overlap on only 23 cases, making it unwise to dismiss one of the datasets in favor of the other, especially when combining elements of the two datasets may be a viable option. Both datasets include data on 20 cases that are not available in the other. Scholars, depending on the demands of their research, will have to evaluate whether the loss of additional variables from the CLEA dataset is worth the addition of 20 extra cases from the CLE dataset.

Why Use Constituency-Level Datasets?

The CLE and CLEA datasets are ideal for research concerned with sub-national variation in party politics. The study of sub-national electoral politics is crucial for understanding how a party system functions, and these two datasets are an important quantitative contribution to this field of research. The datasets are also an excellent resource on minor political parties as they incorporate data on all parties that participate in lower chamber legislative elections, without applying thresholds to the vote count. The CLE and CLEA datasets are more comprehensive than existing electoral datasets in linking voting totals with regional variation.

The CLE and CLEA datasets are valuable resources for scholars interested in party-system fragmentation. The existing literature on legislative fragmentation has tended to focus on the effective number of parties using a formula first developed by Laakso and Taagepera (1979, 5-8). The problem with existing measures of legislative fragmentation is that they tend to focus exclusively on the effective number of parties. While this tends to be a useful indicator for fragmentation within the legislature, it fails to fully depict fragmentation within the party system. For instance, a political party may be regionally prominent but not nationally prominent or a party may be ineffective in legislative politics but prominent in contentious politics. Such is the case in countries like Ecuador and Peru, where regional political parties can play an important role in the fragmentation of political alliances and yet lack significant representation in the national legislature; Brancati's CLE dataset is particularly effective in this instance. Both datasets can be quite useful in providing a more robust measure of party system fragmentation than existing measures of legislative fragmentation by incorporating an additional measure of sub-national variation and including parties that are traditionally cut from data as a result of legislative ineffectiveness.

Conversely, if researchers are interested only in political parties that are legislatively effective then they may want to rely on existing electoral politics datasets (Mackie and Rose 1974, Nohlen 1993, Caramani 2000). The CLE and CLEA datasets are most useful for scholarship interested in sub-national variation or minor political parties, but are no less applicable to cross-national comparison. However, researchers may find the CLE and CLEA datasets overly burdensome if they are simply interested in parties that can directly affect legislation.

Ultimately, scholars will need to judge how and when to incorporate the CLE and CLEA datasets into their research. Thanks to Brancati's as well as Kollman, Hicken, Caramani, and Backer's, careful research and thoughtful organization of their data, these datasets will undoubtedly be of great use to many scholars. With research on party systems and electoral politics increasingly emphasizing the relevance of sub-national politics and the policy influence of minor political parties, the CLE and CLEA datasets are an invaluable contribution to future research.

Notes

[1]The Constituency-Level Elections (CLE) dataset can be accessed at http://www.cle.wustl.edu/.

[2]The CLEA by Kollman et al. currently contains more cross-national data on lower chamber elections.

[3]The Constituency-Level Elections Archive (CLEA) dataset can be accessed at www.election-dataarchive.org

Complete citations for this issue are online at http://www.nd.edu/~apsacp/backissues.html.

Dataset Announcement

CLEA: Constituency-Level Elections Archive

We are pleased to announce a major expansion in the Constituency-Level Elections Archive (CLEA). The central aim of the CLEA project is to produce a repository of detailed results – i.e. votes received by each candidate/party, total votes cast, number of eligible voters – at a constituency level for the lower house legislative elections that have been conducted around the world. This is the largest repository for such data available online.

The directors of CLEA are Ken Kollman (University of Michigan), Allen Hicken (University of Michigan), Daniele Caramani (St. Gallen University, Switzerland), and David Backer (College of William & Mary).

You can access the data at: www.electiondataarchive.org

As you will see, the archive is organized to make downloading easy and to facilitate comparative research. Also on the site are descriptions of political systems and lists of political parties.

The directors gratefully acknowledge the American National Science Foundation (www.nsf.gov) and the Taiwan Foundation for Democracy for their funding of the project. The directors are continuing to improve CLEA and hope to post additional data from more countries and more elections in the near future.

Section Awards

Gregory Luebbert Best Book Award

Award for the best book in the field of comparative politics published in 2008 or 2009:

James Habyarimana
Georgetown University
Macartan Humphries
Columbia University
Daniel Posner
University of California, Los Angeles
Jeremy Weinstein
Stanford University

Coethnicity: Diversity and the Dilemmas of Collective Action. (Russel Sage, 2009*).*

Award Committee Members: Stephen Haggard (Chair), Ray Duch, Randall Hansen

Gregory Luebbert Article Award

Co-winners for the best article in the field of comparative politics published in 2008 or 2009:

Simon Jackman
Stanford University
Shawn Treier
University of Minnesota

"Democracy as a Latent Variable." *American Journal of Political Science* 52 (January 2008).

Award Committee Members: Ruth Collier (Chair), Lily Tsai and Robert Pekannen

Sage Paper Award

Given to the best paper in the field of comparative politics presented at the 2009 APSA Annual Meeting:

Marcus Kreuzer
Villanova University

"Historical Knowledge and Quantitative Analysis: The Case of the Origins of Proportional Representation."

Award Committee Members: Dorothy Solinger (Chair), Matt Kocher and Daniel Ziblatt

Lijphart, Przeworski, Verba Data Prize

Award for a publicly available data set that has made an important contribution to the field of comparative politics:

Mark Tessler
University of Michigan
Amaney Jamal
Princeton University

For the *Arab Barometer*

Award Committe Members: Bo Rothstein (Chair), Jose Cheibub, David Cingranelli

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