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Letter from the President What Does Political Choice Mean?

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Politics involves making choices; if there is no choice, there is no politics. We debate why political choices are made as they are (though we often confuse the why and what with how, taking the process as the substance of the choice). An important element of choice involves agency¹ or a sense of the locus of initiative or voluntarism. Agency may, in our arguments about choice, be high or low.

We differ among ourselves about how much intentionality, deliberation, awareness, and choice we assume in our explanations. Some of us see great purpose and drive in political action: people "design" institutions, or "delegate" or "monitor" an agent. Others see action but not much intention, doing things not because of purposefulness, but because of appropriateness, or custom or habit.

Thinking about our interpretative attitudes toward choice and intentionality may clarify what we mean by explanations in politics. The lower the degree of choice, the less politics is at work. Other social processes (socialization) or non-social ones (geography) may be operating, but not politics. The item of difference among these processes is the degree of constraint: powerful constraints mean little room for political action.

That may produce some paradoxes: the more laws we find that drive regularities in political action, the more constraints on individual discretion we find, and the less we can say choice is at work, thus diminishing the role of politics.

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In his deservedly famous book *Guns, Germs, and Steel*,² Jared Diamond provides a breathtakingly sweeping interpretation of the grand trajectory of human development. "Why are your people on top of my people?" asks of Diamond a man from New Guinea who helped with the research. Diamond judged this man to be as smart and competent as anyone he knows in the richer first world, so it can't be race, he concluded, in a superbly succinct demolition of racism. The answer lies in geography: the east-west contours of moun-

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tain ranges, deserts, and jungles shaped the spread of grain-growing as well as the use of animals not for heavy work, and the pig-chicken-human interactions that generated resistance to disease. The book is a brilliant integration of variables from biology, geology, ethnography, and a host of other research traditions.

In this process, are there any human choices being made? Diamond's treatment does not examine any decisions. Stuff happens, a passive voice with no agency. Indeed, therein lies the power of Diamond's argument: certain races overwhelmed their neighbors because they happened to be living in the right place when the domestication of plants and animals was "discovered," because the mountains lined up just so, and not because of superior attributes in genetics or of culture or social organization or of institutions. By poking hard we can find some decisions in there – someone had to plant the seeds instead of picking berries, train the animals instead of eating them, create irrigation, organize the group, and others had to decide if they would join that new activity, or continue as hunter-gatherers.

There are certainly some choices being made at the micro level. Some selection process clicks in: those who domesticate raise more food, and spread in numbers. In this mechanism, the choices are not social and not self-aware. As with evolutionary models and genetic models generally, these arguments require no consciousness, nothing we can consider to be choice, no politics.

This helps explain Diamond's greatest weakness for specialists in comparative politics: why do countries within each racial/cultural zone diverge? Among the Caucasians, why

is Germany economically advanced, Albania not? Diamond cannot explain this within his geographical determinism. In a subsequent book, *Collapse*,³ he explores this with vivid case studies of failure, the commons problem, such as who cut down the last tree in Easter Island, why was it cut, and why did they not stop before the end? The power of culture is vividly illustrated with the example of Greenland: the Norse left or died when the climate turned cold because they "could not" or "would not" (a huge difference) learn what the native peoples had mastered in hunting seals. So a choice is made there, but we don't know why or by whom.

At the farthest end of the spectrum – this is a continuum about high vs. low choice – from Diamond are "great man" theories: things happen because of decisive choices by specific individuals – Hitler, Mao, Roosevelt, Stalin. The great men or women seem to appear in "interesting times," and disappear from the dull periods, which is not surprising as there is more opportunity for leadership in the periods of war, revolution, and depression, which shatter established structures. In our time, with economic growth so centrally the dependent variable of concern, Lee Kwan Yu is perhaps the most striking figure compelling attention in causal arguments: why do some authoritarian leaders choose development as a goal, while others like Marcos become kleptocrats? Most incentive-based theories assume predation by rulers, so a pro-development utility function seems harder to explain. Authoritarianism allows leaders to choose, but why do they choose one path and not the other?

Between geographical determinism at one end and great women and men at the other, lie many things that absorb our attention: institutions, culture, social structure, and the interna-

tional system, among others. Institutional “design” analysis poses interesting challenges for the location of choice and agency. At first blush, institutional analysis lies squarely in the high agency, high choice side of the continuum. Rational-choice institutionalists see structures created by people to solve problems: to prevent cycling, to solve problems of information, to facilitate coordination, to create focal points for decisions, structuring anarchy in various ways to overcome problems of collective action. Cox, for example, studies how rules in the House of Commons evolved to enable decision-making: without rules, members’ objections paralyzed the House; rules limited debate in a patterned way to “force” decisions.⁴ For this to happen, enough members had to want decisions to outvote those who feared losing their unit veto possibilities. So we can see the choice here, the self-interest, the rationality in choosing to accept the rules. Institutions are designed to solve problems, thus chosen. After that, the institutions are constraints: members are not so free to choose. They act within the constraints thereby imposed. They act strategically within the confines of the rule. If they stray outside those constraints, they may be committing political suicide. The rules survive only if a great mass of people supports them; they are self-enforcing in this sense. If there is strong criticism, rules can change, as happened with reducing the power of the House of Lords in the early 20th century.

The question of choice highlights a distinction for institutional analysis, that between the formation of institutions and the consequences of a specific institutional form. At the time of formation, there is a great deal of choice and agency. In advocating one institutional arrangement over another, actors behave quite strategically in pushing for the system that

maximizes their power for the longest time. The idea is to lock in the most favorable arrangement and then hold it.

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This is far different from a Durkheimian or Burkean or Tocquevillian view of institutions which stresses tradition, customs, and habit over time: a logic of appropriateness, not of instrumentality. For the rationalist approach, once institutions are established, the element of choice fades away. Analysis focuses on the consequences of the rules, not on how the rules are contested. It is the effects that matter. Rogowski, for example, argues that PR electoral systems have higher prices than plurality ones, as the latter favor consumers while the former favor producers.

Constitutions “lock in” a set of rules for selecting and rotating leaders. The politicians who deviate are turned out of office, unless they manage a coup and become authoritarians. Voters have a choice on whom to vote for, but the effects of their choices, aggregated, are refracted through institutions. Variance among countries in policy output comes not from vari-

ance in preferences, but in institutions. It is structure which shapes policy outputs, not preferences or coalition building.

So institutional design, a quintessentially choice-centered mode of thought, leads to the absence of choice as time goes by. To be a powerful explanation, the institutional constraint has to bite hard: people cannot escape their force. The interest in path dependence (Pierson)⁵ springs from this: that is, the past can lock in earlier choices, depriving the current generation of the same degree of freedom. An example can be found in the debate over the role of “legal family” (civil vs. common law) in shaping systems of corporate governance. LaPorta et. al argue that legal family⁶ determines the level of protection from exploitation given to minority shareholders by insiders, and the degree of protection determines the corporate governance outcomes of diffuse shareholding, as in the US, versus concentrated blockholding, as in most other countries. The choice of legal family reaches way back into the past (the 12th century for the UK and common law, the French Revolution for the civil law tradition). Countries rarely change legal family, a less likely occurrence than rewriting the constitution. So having locked into a legal family, this explanation assumes there is no further choice to be made, and hence no politics. But legislatures pass laws on legal family, and corruption and law enforcement, or its lack, are not determined by legal family, but by other political processes. This is, at any rate, an example of taking potentially a high choice item (picking legal family) and turning it into a low choice item through path dependence.

The notion of strong laws governing human behavior undermines a notion of choosing. If the self-enforcement is quite powerful, we have the paradox

of strong choice in institutional design, but weak choice in institutional constraints. The power of institutional arguments is that they constrain. The more powerful the constraint, the less choice people have in the present. Thus, the stronger the impact of institutions, the less the element of choice, which changes the meaning of politics.

”[...] the stronger the impact of institutions, the less the element of choice, which changes the meaning of politics.”

Do institutions really lock in? Rebellions do occur. So we need ideas on why people do or don't comply, an old debate about legitimacy and coercion. The more settled the institutions, the harder it is to overthrow them. Are institutions “self-enforcing” because people accept the outcomes and the procedures, or because people feel hopeless when confronted with the costs of initiating change? The Soviet Union had lots of coercive instruments. Its collapse questions the power of “institutional strength” arguments of the kind Huntington made in *Political Order in Changing Societies*.⁷ The cases of strong institutions in that book were Chile, Lebanon, and the USSR, all of which subsequently collapsed!

Civil war and revolution are the pivot points in these arguments, moments when institutions are challenged⁸ and the new “design” of institutions takes place outside the structure of formal ones, presumably by “meta institutions.” The element of choice looms large in Laitin and Fearon's analysis of ethnic conflict. Why do people liv-

ing side by side suddenly start killing each other? A small amount of information, a lot of uncertainty and potentially great danger spawns aggressive offense-defense reaction. It is a tipping point argument, where the equilibrium suddenly shifts. There is a clear mechanism noted: specific actors spreading “misinformation” among vulnerable people. Substantial agency and choice are at work here.

In his well-known treatment of African political economy, Bates⁹ shows leaders favoring city consumers of food over the country producers. The logic seems clear: policy chosen with the goal of staying in power, a bargain between the rulers and the urban masses. The mechanism at work is a kind of punishment: if they don't win over the urban residents, they lose power. It is not the median voter, but the median rioter, worker, protester, each having their own choice about whether to exercise their particular form of power. Just how is that bargain constructed? Process tracing documents the specific sequence of events that produce it, and the agency of each participant. Iversen and Soskice seek the political foundations of capitalism's varieties in political institutions which themselves are traced back to social structures that precede democratization and industrialization.¹⁰

“The less ‘law oriented’ we are in the search for arguments, the more obvious the role of choice.”

The less “law oriented” we are in the search for arguments, the more obvious the role of choice. Some researchers stress contingency, leadership, the creation of solutions

where the incentives are underdetermining – potential rather than reality. Culpepper¹¹ argues this about corporate governance: change in French law opened an opportunity, but it might have gone unused, without the leadership of specific people who redefined the terrain through their action. Tiberghien stresses leadership,¹² Herrigel stresses context.¹³ Berger attacks the determinism of ideas about economic convergence, arguing that firms have choices to make.¹⁴ So actors are given “free will” here in opposition to the “determinism” of institutional incentives. But where do they get their “utility functions” and how much choice do they have, how much freedom from socialization or genetics (Fowler)?¹⁵ What of culturalist or constructivist mechanisms of causation? At first it seems these ideas are all about expanding our notion of choice, as much of the work criticizes purely materialist or objectivist notions of utility functions, arguing that people construct an identity that frames their actions. So people have choice among their various “roles” or identities: worker, ethnic group member, town or national citizen, churchgoer, party militant. If these roles shape political behavior (voting, protest), a key decision is which identity to choose. But therein lies the problem: what is the mechanism of identity, construction or choice? Is it infinitely malleable, constructed out of any elements at all? That seems unlikely, as people are born and raised into structures and circumstances. Ethnic conflict based on essentialism implies no choice – ancient hatreds, as constraining as institutionalist path dependence. Laitin and Fearon reject essentialism in favor of “situational context.” This increases choice, but the situation is nonetheless quite constraining.

The recent surge of interest in genetics and neuroscience seems to cut

strongly against the notion of choice. If twins separated at birth have a tendency to vote the same way, do they have any choice over how they vote? Tetlock¹⁶ separates forecasters into “foxes and hedgehogs” and finds the former do better at prediction. These also correlate with political tendencies toward liberal or conservative views. How does an individual happen to be one or the other? If that is also heritable (genetics) or strongly socialized, then again choice fades away.

“By finding the rules that constrain individuals, we risk losing sight of the dynamic processes whereby solutions are created, problems solved, or failures take place.”

By reflecting on what we mean by choice, we can clarify causal mechanisms at work in politics, and what we mean by politics. Politics without choice is not politics. It is important, as are rainfall and earthquakes, mountains and oceans, but if there is no one making choices and decisions, do we really have politics? By finding the rules that constrain individuals, we risk losing sight of the dynamic processes whereby solutions are created, problems solved, or failures take place. If we look at these things, we make our analyses messy. And we disagree among ourselves on just where to find the balance between parsimony and accuracy, between laws about many events and cases or analytic narratives. Clarifying what we mean by choice, where it happens and where not, is one way of sharpening the points of

debate among us, and that is a good thing for a vigorous Section on Comparative Politics.

Notes

¹This passage draws on material from my forthcoming essay “The Role of Politics in Economic Development”, the *Annual Review of Political Science*, Margaret Levi, ed., 2008. 11:137-59. <http://polisci.annualreviews.org>.

²Jared Diamond, *Guns, Germs, and Steel* (New York: Norton, 1997).

³Jared Diamond, *Collapse* (New York: Penguin, 2005).

⁴Gary Cox, *The Efficient Secret* (Cambridge University Press, 1987). For a critique of cycling arguments, see Gerry Mackie, *Democracy Defended* (Cambridge, 2003).

⁵Paul Pierson, “Increasing Returns, Path Dependence, and the Study of Politics,” *American Political Science Review*, 94:2, 251-67, 2000.

⁶La Porta et al. “Legal Determinants of External Finance,” *The Journal of Finance*, 52:3, 1131-1150, 1997. For criticism of this view see Peter Gourevitch and James Shin, *Political Power and Corporate Control* (Princeton University Press, 2005) and the writings of Mark Roe at Harvard Law School.

⁷Samuel Huntington, *Political Order in Changing Societies* (New Haven: Yale University Press, 1968).

⁸Barrington Moore, Jr., *Social Origins of Dictatorship and Democracy* (Boston: Beacon Press, 1967). This book is about revolution and change; Huntington’s is about order and institutionalization.

⁹Robert Bates, *Markets and States in*

Tropical Africa (Berkeley: University of California Press, 1981); James D. Fearon and David D. Laitin, “Explaining Interethnic Cooperation,” *American Political Science Review*, 90: 4, 715-735, 1996.

¹⁰Cusack, Thomas, Torben Iversen and David Soskice. “Economic Interests and the Origins of Electoral Systems,” *American Political Science Review*, 101: 3, 373-391, 2007.

¹¹Pepper Culpepper, “Institutional Change in Contemporary Capitalism: Coordinated Financial Systems since 1990.” *World Politics*, 57:2, 173-99, 2005.

¹²Yves Tiberghien, *Entrepreneurial States: Reforming Corporate Governance in France, Japan, and Korea* (Ithaca: Cornell, 2007).

¹³Gary Herrigel, “Corporate Governance: History without Historians” in Geoffrey Jones and Jonathan Zeitlin, eds. *Handbook of Business History* (Oxford: Oxford University Press, 2007).

¹⁴Suzanne Berger, *How We Compete* (New York: Doubleday, 2005).

¹⁵James Fowler, “The Genetic Basis of Political Cooperation,” in James H. Fowler, Laura A. Baker, and Christopher T. Dawes, 2007 unpublished mimeo <http://www.polisci.umn.edu/pecolloquium/Fowler.pdf>.

¹⁶Philip Tetlock, *Expert Political Judgement: How Good is It? How Can We Know?* (Princeton, 2005).

Note:

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

Big, Unanswered Questions in Comparative Politics

Introduction

This issue's symposium departs radically from our usual format. A typical symposium in *APSA-CP* includes three to five essays of approximately 2500 words each. Following a suggestion from Editorial Board member Debra Javeline that was enthusiastically endorsed by the Section's Executive Committee, we decided in this issue to ask about *thirty* comparativists to pose a big unanswered question in comparative politics, and to do so in 300 words or fewer. We told them that it should be either a question on which no one is working as far as they know, or a question that has been studied but never been answered satisfactorily.¹ It should also be a question that would interest many of our readers. Beyond these instructions, they were completely free to exercise their creativity. Their questions could be conceptual, empirical, or theoretical; normative or positive; about a general tendency or a specific event.

“We were looking for some bold, creative thinking about genuinely puzzling topics.”

The challenge was deliberately daunting. We were looking for some bold, creative thinking about genuinely puzzling topics. Not knowing exactly what to expect, we trusted in the ingenuity of our colleagues, but also in the safety of numbers: thirty (we actually got essays from twenty) scholars should be able to think of at least a few great ideas! It was also a large enough number of thoughtful people to aim for a balanced repre-

sentation of scholars by research topic and regional specialization. Moreover, we limited our sample to one person per department. Is the result an unprecedented snapshot of what is on the minds of leading scholars in our subfield? You be the judge.

Some of our contributors posed fundamental questions about the methodology of comparative politics. Why are our answers so weak (Zuckerman)? Does (geographic) size matter (Taagepera)? Is comparative politics methodologically distinct from American politics (Lewis-Beck)? How should we select our questions to begin with (Alexander)? Two contributors focused on gender. Karen Beckwith found a big question hiding in plain sight: “Why do men dominate politics?” while Michele Penner Angrist was curious about the association between authoritarianism and the treatment of women in the Middle East. A couple of questions (from Casper and Lichbach) call attention to civil war, which is certainly a relatively new and burgeoning field of research in both comparative politics and international relations. [See our Summer 2007 symposium for some policy implications of this body of work.] A cluster of questions came from political economy or public policy, although within this broad category they are quite diverse. Mahoney and Steinmo ask why some countries grow faster than others; Bratton and Hunter worry about social and political inequality in the developing world; Migdal calls attention to some important, overlooked activities of states; and Birch begins what will surely become a long conversation about the politics of coping with climate change.

However, most surprising and intriguing was the convergence of six contributors on questions about the cog-

nitive, subjective, cultural, or ideational dimension of politics. The questions posed by Anderson, Estévez-Abe, Kitschelt, and Rosenbluth overlap significantly: What goes on in actors' heads as they make decisions? External incentives do not seem to explain behavior sufficiently well. If these scholars have identified the next wave of research, it appears to borrow from psychology. O'Brien and Van Cott overlap these questions somewhat less, but they share skepticism toward generic, one-size-fits-all theories and a willingness to explore the impact that leaders and their unique personal qualities can have.

Our sense is that some of the questions that follow are bigger than others, and some have been addressed more satisfactorily than others. We expect that our readers will be eager to remind the subfield of their own research that addresses some of these questions. We would welcome such submissions for a future issue. Nevertheless, we also think that quite a few of these brief essays are onto something new and original, and we hope that other researchers will begin to investigate and *answer* the questions they pose.

Notes

¹ When one potential author objected that *no science ever answers any question “satisfactorily,”* we clarified: “There are many questions that are intriguing enough to be good dissertation topics, and for many of them there are plausible answers, debates, something substantial to build on. We don't want those. Rather, we're looking for questions that have been so neglected or studied so poorly that one might as well start from scratch as try to build on previous efforts to answer them.”



Pictured left to right, from top, Gerard Alexander, Chris Anderson, Michele Penner Angrist, Karen Beckwith, Sarah Birch, Michael Bratton, Gretchen Casper, Margarita Estévez-Abe, Wendy Hunter, Herbert Kitschelt, Michael S. Lewis-Beck, Mark Lichbach, James Mahoney, Joel Migdal, Kevin O'Brien, Frances Rosenbluth, Sven Steinmo, Rein Taagepera, Donna Lee Van Cott, and Alan Zuckerman.

Conflict

What explains the intensity and outcome of civil conflict?

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What explains the variations in outcomes and magnitudes of civil conflict? For example, why do some conflicts result in peaceful settlements while others lead to civil war? Why can the same type of event, such as political protest, be peaceful in one context but violent in another? Over the last twenty years, comparative scholars have focused on connections and transformations within spe-

cific types of conflict. Scholars who studied authoritarianism began to explain why some democratization processes resulted in democratic consolidation while others ended in democratic collapse. Social movement theorists expanded their focus to include contentious politics more broadly. People studying insurgency groups included civil wars in their research. Yet, these types of conflict – democratization, contentious politics, and civil wars – are related to one other, and to other examples of civil conflict. International relations scholars have recently incorporated domestic politics to increase their understanding of international political behavior, including conflict.

Similarly, civil war has become a prominent focus among international relations scholars, as the distinction between intra- and inter-state violence is seen as more artificial than was once thought. In a complementary way, we comparative scholars can enhance our understanding of domestic political behavior by comparing types of civil conflict: locating specific types of conflict (such as democratization, contentious politics, and civil wars) within the general space of civil conflict, observing the relationships between them, and explaining the variations in outcomes and magnitudes of violence.

How does contention among states causally connect with contention within states?

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During the 20th and 21st centuries – I will call them century 20-21 – contending constellations of historical forces – carrier groups and their associated ideas and institutions –

have advanced alternative internal/domestic and external/foreign policies. The resulting struggle among rival blueprints for organizing international and domestic systems gives the world politics of any era its particular dynamics.

For example, the interstate conflicts and internal wars of the 1920s and 1930s involved the clash of democracy, fascism, and communism. In the post-WWII period, the old colonial and imperial order was opposed by new states seeking national liberation from empire. During the Cold War, authoritarian communism fought with democratic capitalism. During the 1960s, 1970s, and 1980s, states in the West sought different ways to organize their democracies and markets. Various permutations of liberalism and neoliberalism and conservatism and neoconservatism found domestic and global champions. Since the Iranian revolution, political Islam has emerged as an alternative to structuring states. In the post-Cold War world, American neoliberalism collided with alternative neo-capitalisms and neo-socialisms opposed to American hegemony. In sum, the politics of century 20-21 involve competing frameworks for constructing and reconstructing states. Bloody battles over democracy and dictatorship, markets and planning, nationalism and liberalism, and international institutions and global anarchy have resulted.

External and internal competition, both peaceful and violent, must be related. One reason is that state leaders attempt to stay in power by fostering their state's internal and external sovereignty. Another is that the sovereign state looks outside of itself for models of political-economic organization. Hence we arrive at perhaps a major continuing problem of world politics: In a particular world-historical era, when states face choic-

es among grand strategies and paths of development, how does the contention among states causally connect with contention within states?

Political Economy/ Public Policy

How can we better capture the concept of clientelism?

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The theoretical turn in comparative politics towards a new institutionalism has granted undue privilege to formal rules. The sub-discipline is replete with studies on the effects of constitutions, electoral systems, and party systems on political behavior. This preoccupation is justified for stable democracies with a rule of law; but it is misplaced for the range of regimes in the developing world where the exercise of political authority can be arbitrary and where constitutionalism remains contested. In these regions, politics are usually played out according to the informal, unwritten practices embedded in patron-client ties. Although analysts routinely pay lip service to patronage and clientelism, these terms are rarely conceptualized carefully or made operational for purposes of empirical research. Moreover, the "high" politics of elite patronage continue to receive greater attention (see the literatures on presidentialism, political machines, and corruption) than the "low" politics of everyday clientelism. So, my candidate for a small but important unanswered question is: how can comparative scholars better capture the concept of clientelism? If we define clientelism as the expression of political deference in return for material rewards, then a political economy

perspective is likely to reveal more than an institutional approach. As a conceptual starting point, we must distinguish "clients" from both "subjects," who submit to the will of rulers on non-economic grounds, and "citizens," who claim individual rights in order to hold leaders accountable. In new democracies, regime survival rests in good part on the willingness of citizens to resist the arrogation of power by political elites. Yet researchers lack instruments to distinguish clients from subjects or to track transitions to citizenship. If clientelism is as pervasive in the non-Western world as the comparative literature would lead us to believe, why do we still lack an objective set of criteria for reliably distinguishing clients from other sorts of political actors?

What are the political repercussions of economic inequality in Latin America?

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Latin America has long been one of the most unequal regions in the world. Whether measured by income or access to services, inequality (a measure of distribution) remains high in most countries. In some, it has increased despite reductions in poverty (an absolute threshold below which basic needs are considered unmet). In recent years technocratic policy circles have subordinated concerns about how wealth is stratified to a focus on alleviating poverty and assuring minimal levels of social protection. Theoretically-oriented students of comparative politics (e.g. Carles Boix 2003) have taken up inequality as a central concern but some of their most basic schematic ideas run counter to what has occurred in Latin America. Against the

prediction that high inequality impedes democratization, Latin America has witnessed a sustained unfolding of democratic regimes amidst unprecedented levels of economic inequality.

The coincidence of democratization and high inequality in Latin America calls for a research agenda that takes inequality seriously and yet investigates its effects on political outcomes other than democratization, procedurally defined. Do greater social distances among citizens heighten feelings of unfairness and inefficacy, which in turn diminish the perceived legitimacy of a country's political institutions? Do they decrease the frequency with which people seek and gain access to existing institutions that are nominally open to them – e.g. formal legal rights and “universal” health care – and if broadly utilized would enhance not only their own well-being but create further investment in the system? Do they increase the incidence of corruption by inflating elites' sense of superiority and impunity? What kinds of inequality (e.g. income vs. services) and which patterns of distribution (despite similar GINI coefficients) – are most pernicious in impeding the development of a culture of citizenship? These and related questions yield testable hypotheses that await empirical investigation.

Why have political scientists virtually ignored some sorts of huge projects, and how do states shape their citizens?

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I have spent most of my career studying states and their relations

with their populations. What brought me to that subject a quarter of a century ago, I think, was the pure audacity of states. Their leaders' ambitions for them seemed almost limitless: recruiting people to die for them, collecting huge proportions of the wealth people produced, using massive doses of force and violence against their own populations and others, embarking on the most massive building projects in history, and much more.

In terms of comparative politics, some grandiose state projects, such as war, policing, and social welfare, have received considerable academic attention. What puzzles me, though, is why other central state activities, such as taxation and mass education, have generated only a moderate amount of research (although work on taxation is picking up). And even more inexplicable is that still others have received almost no attention. Take the politics of building and creating, for example. Why do such obvious topics receive such middling or piddling attention from comparative political scientists? After all, between them public funding on education and building projects constitute a hefty proportion of state expenditures, especially of discretionary, non-entitlement programs. I have particularly wondered about the neglect by political scientists of another area of state audacity: the efforts to shape how citizens look and sound as well as the tenor and tone of relations among them. In some states, this sculpting of individuals as they appear in public is pretty obvious, as in post-revolutionary Iran, where women had to be covered and men appeared with beards. Even in countries such as the United States, though, the state has played a critical role in creating the representation of the “real American” through such means as vagrancy laws, school dress codes, official and unofficial English-language policies,

and much, much more. Could political scientists begin exploring comparatively the shaping of the citizenry?

What are the causes and consequences of the recent rapid economic growth in India and China?

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What has caused recent rapid economic growth in India and China? How equitable has it been? Can these cases be treated as broadly comparable examples of new models of development in the contemporary global economy? Does understanding the sources of their recent growth provide insight into the likelihood of future growth in other countries? The answers to these questions have immediate implications for the lives of hundreds of millions of people and for the basic organization of the global economy. To be sure, some comparativists are now conducting important work on development in both India and China. But even more needs to be done toward arriving at the full range of plausible answers. Compared to what we know about the possible explanations of post-World War II development in Korea and Taiwan, for example, we fall far short for India and China.

In comparative politics, it is true, big questions are never definitively answered to everyone's satisfaction. Yet it is possible to reach partial and compelling answers that inspire further work and the accumulation of knowledge. Earlier comparative research on the East Asian “miracles” used comparative-historical evidence to arrive at explanations that challenged orthodoxy and that later influ-

enced broadminded policymakers. So too might future research on India and China hold the key to understanding which models of development could possibly enhance human welfare in the contemporary world economy.

How can governments best cope with the economic, social and political effects of climate change?

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Increasingly recurring floods, droughts, forest fires, hurricanes, and changes in the geography of contagious diseases are just some of the effects already being experienced as a result of anthropogenic climate change. While intergovernmental coordination at the global level is the main focus of efforts to mitigate the extent of climate change, it is at the national level that its effects are largely dealt with in practical terms. In adapting to and managing the impacts of climate change, governments face a difficult challenge. Considerable investment in technology and infrastructure is needed in order to enable states effectively to transition to low-carbon economies. Such investment need not be economically or socially debilitating when economies are in steady-state, yet the natural disasters accompanying climate change, combined with the rising cost of natural resources, mean that many economies are beginning to feel the pinch. Economic downturn threatens to make transitional policies prohibitively expensive, resulting in difficult choices. Investing in the means of reducing carbon emissions (through renewables technologies and energy efficiency drives) and dealing with the aftermath of climate

change-induced disasters are both costly, and the resulting burden has the potential to lead to social and political instability. The social and political ramifications of the choices governments make in this sphere may well come to dominate politics in the years to come, yet political scientists have been slow to take on this topic as a serious object of study.

Why is the West so rich?

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Excluding a few petro-states, twenty-seven of the richest thirty countries in the world today are Anglo-European. Why? In other words, what is it about “the West” that has made it so rich and successful? While this question has obviously been a subject of great interest to social scientists in the past, political science has backed off this fundamental issue in recent years precisely because it is “too big.” Europe, a rather small peninsula on the western edge of the great Asian land mass has had its share of ethnic conflict, brutal dictators, epidemics and genocides – all reasons used to help explain why development is so difficult elsewhere. Yet Europeans (or the West) somehow developed cultures and institutions that helped them conquer the rest of the world. Clearly, the early development of democratic capitalism gave European societies economic and technological advantages. But this fact simply begs the question: What was it about European society or culture that led these peoples to develop democratic capitalism? Many today reject Weber’s classic explanation as overly simplistic, too Euro-centric, and/or just wrong. But do we have any good alternative hypotheses? I submit that having a better understanding of how and why Europe (or western society) devel-

oped might shed light on why others developed differently. This does not suggest that there is only one route to development. Quite the contrary. But if we do not or cannot deal the question of what made the West so rich, how can we hope to explain development at all?

Methods

How, how much, and why does the size of a country affect its politics?

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Is politics different in small and large countries, all other factors being the same? In their seminal *Size and Democracy* (1973), Robert Dahl and Edward Tufte showed that this is so, and should be so on logical grounds. Amazingly, no other English-language book on size effects has followed. Later research shows that smaller countries have fewer parties but more party members per capita. They have smaller assemblies but larger foreign trade. They just may have more durable cabinets. Empirical studies of *whether* size matters still are few, *how much* it matters are fewer, and *why* it matters (to the specific extent it does) has hardly been asked at all, except for assembly sizes and trade. But without normalizing for size effects, we may mistakenly ascribe such effects to other causes. For example, small countries are bound to have larger foreign trade, which often is attributed to their supposedly more open attitudes. First-past-the-post elections are used mainly in ex-British democracies, which mostly happen to be very large (India, US, UK) or very small (Caribbean and other islands), while stably democrat-

ic PR countries are mostly in between. Unless one accounts for size, one may draw erroneous conclusions about the consequences of electoral systems. The “Size and Politics” chapter of my *Predicting Party Sizes* (Oxford UP, 2007) reviews how things stand, but no one seems to be actively working on size.

If comparativists seek to answer questions, why are their answers so weak?

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Before offering new or overlooked big questions for analysis I suggest that the subfield should improve its set of available answers. Today, the ideal study in comparative politics is an observational study designed to answer an explanatory question. In this effort, the scholar designs and applies research to describe the case or cases at hand, strives to include quantitative and qualitative data and employ appropriate analytical techniques, evaluates possible solutions, and then answers the question. Neither of the favored sources for answers – process tracing and out-of-date versions of rational choice theory – is up to the task. In the former, the political scientist somehow uncovers the explanation by looking at the evidence, as though perceptions of the world were independent of concepts and theories. The latter ignore bounded rationality, behavioral economics, and decision theory, not to mention neural sciences and theories that do not include instrumentalist assumptions of choice. Poor answers also consider only simple one-directional causal claims, when we well know that the political world contains multi-level endogenous phenomena. Better answers will come from reading widely, thinking hard, and testing

explanatory principles in laboratory studies and field work and with agent-based models. Research will pose, refine, test, keep, and discard explanatory principles. Thinking about the answers before applying them to the problem at hand is still a necessary step in solving the subfield’s puzzles. I don’t think that we’ve run out of questions. Rather, I think that we’ve run out of answers.

Is comparative politics methodologically exceptional?

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Do comparativists do political science differently? There is much debate. But a general inspection of leading research hints the “methodological exceptionalism” of comparative politics is less than self-evident. With James Krueger, I content-analyzed the methods in the articles of the *American Political Science Review*, the *American Journal of Political Science*, and the *Journal of Politics* (1990-2005), N = 1756 papers. We grouped the papers into subfields, here focusing on the two largest, American = 1199, Comparative = 422. Are methodological differences observed, in terms of these criteria: 1) argumentation; 2) model evaluation; or 3) data? (For a fuller discussion of these criteria, see Krueger and Lewis-Beck, 2005; Krueger and Lewis-Beck, 2007). Concerning argumentation, we identify three kinds: equations, verbal, qualitative. These argumentation styles vary in the precision with which they develop theory. Classically, the most precise expression of theory may be its articulation in an equation, followed by statistical testing. [Blalock, 1969 was among the first to introduce this notion of theory construction, essentially adopting an

econometric view. For a current perspective upholding this notion, see the discussion by econometrician Gujarati (2004)]. In our classification, the “equation” approach refers to presentation of a formal equation accompanied by statistical tests; the “verbal” approach refers to informal verbal presentation of theory accompanied by statistical tests; the “qualitative” approach refers to verbal presentation of theory without accompanying statistical tests. Here are the findings: Comparative – 37.0% equations, 55.4% verbal; 7.6% qualitative; American – 29.4% equations, 66.2% verbal; 4.3% qualitative. From this sample, Comparative does not appear less methodologically precise than American.

Now consider the question of model evaluation. For Comparative, 41.7% of all the papers do not report a fit statistic; for American, it is 37.0%. Among those studies that do, some version of the R-squared is the top choice: in American, 49.5%; in Comparative, 54.0%. Consider statistical significance tests. The percentage of American papers versus Comparative papers reporting no statistical significance tests is virtually indistinguishable (32.3% to 32.7%, respectively). Lastly, consider the data. Overall (N = 1756), surveys were most used, followed by time series. For American, the percentages here are surveys = 35.1%, time series = 29.0%; for Comparative, the percentages here are surveys = 33.1%, time series = 22.6%. Again, they are very close.

Is comparative politics methodologically exceptional? Perhaps not so.

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How should comparativists select "big" and "small" unanswered questions?

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My big question concerns not any one research project but rather how as comparativists we should generally select our research projects. A symposium to identify big questions suggests an ample existing supply of small ones being worked on. Several factors may have been pulling us in this direction. Projects of a more incremental nature often arise when researchers are testing the margins of very mature research programs. Incrementalist projects also follow when project selection is driven by incrementally improving research methods. Researchers can also be drawn to incrementalism when they do not believe that more ambitious projects are feasible or desirable. None of these are persuasive justifications for tackling so many small questions at the expense of larger ones at the current time. No current

research approach, including institutionalism, is so credible and effective that its outermost margins deserve to be populated with what amount to many dozens of micro-tests. Moreover, research should not be methods-driven, and researchers should not be bashful, when the intellectual challenges before us call for exploring large pieces of political reality. For example, we now face many of the same questions confronted by both modernization theorists and their critics: What causes nation-states to cohere instead of fragment? What causes disparities in economic growth? What are the consequences of varying degrees and types of economic change? What explains variation in the degree of effective governance? To what degree should we expect regime types to converge? If a range of muscular studies addressing these questions were accompanied by incrementalist studies spun off by them, comparative politics would be both more interesting and more policy relevant.

Culture/ Subjectivity/ Cognitive Issues

What role do individuals play in collective action?

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How can we incorporate individuals into our understandings of collective action without abandoning the goal of building middle-range theory or descending into historical narrative? By "individuals" I mean not only the microfoundations of behavior, but particular people with all their idiosyncrasies and biographical quirks.

Although some scholars (e.g. James Jasper) have called for more attention to biography, and there is growing interest in protest leadership, ours is still a rather bloodless approach to contention. Yes, studies of protest often include exciting narratives that remind many of us why we became interested in politics, but these narratives are typically presented as stylized cases rather than rounded stories that help pinpoint unexplored assumptions and uncover where our theories have gone too far. Of course we must do some distilling, but at the same time we can revel in the particular, to see where it raises questions about what we think we know. This is thus not so much an alternative to the search for general laws as a stepping stone to better middle-range theory – theory with clearer scope conditions. It implies less suppressing of what we've learned from talking with protesters (leaders and followers), targets, and third parties; and not ghettoizing "descriptive" accounts of protest episodes in area studies journals, outside "real" political science." We've reacted too strongly against "great man" theories of history and the obscurities of personal and social psychology, and have lost track of the people who make up our stories. We need more attention to the sweaty, unfiltered detail of protest, and special focus on those occasions when people act like (theory says) they shouldn't act.

How can we better understand the subjective dimension of politics?

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People are at the heart of politics. What they think and do is critical for understanding elections, revolutions,

legitimacy, ethnicity, corruption, and economic redistribution – any number of subjects linking citizens' desires and government action. If true, a big unanswered question in comparative politics is how we can better understand the subjective dimension of politics. Answering it would require accounting for beliefs in terms that are culturally specific and socially shared (constructing so-called emic accounts), but also descriptive in terms that can be applied to other cultures or contexts (etic accounts). In short, a long-standing, but unanswered question has been how we can meld emic and etic approaches to mass politics. Currently, the two exist in parallel methodological and substantive universes. One (the emic approach) inhabits the world of cultural politics, uses the intellectual and methodological tools of anthropology, and usually focuses on politics in the less developed countries. The other (the etic approach) exists as a sub-field of behavioral politics, employs the technology of random sample surveys, and pays more attention to politics in the rich societies.

Though rarely intersecting, these sub-fields share a passion for subjective politics and view people as social beings. In a world of migrating people and ideas, a focus on subjective politics should be able to tell us (from an emic and etic perspective) what people's understandings are, and how they arise, come to be shared, and motivate action. Answering this question would help us to go beyond entrenched ways of thinking about culture, values, and norms. It would help us understand, for example the diffusion of understandings of human rights and democracy, the construction and consequences of identity, the mobilization of social groups within and across borders, or the rise of supra-national and cell-based, non-hierarchical politics that transcend or lie beneath the nation-state.

What is the relationship between leadership and culture?

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Leadership is among the most slippery concepts in comparative politics. We have done a poor job defining it consistently or operationalizing it in ways that facilitate hypothesis testing or advance our understanding of its sources and impacts. Many scholars reject the concept entirely as a residual category for what we cannot measure. But others have argued persuasively that leadership is among the most important factors in determining the variation in outcomes of political events and processes, particularly the design and functioning of a range of political institutions. What do we know? We know that leadership encompasses some personal qualities, many of which are innate to outstanding individuals, while others can be acquired through training and experience. We know that the exercise of leadership is contingent upon the context in which it is exercised. Variations in the incentives that institutions provide, the level of government, the emergence of economic or political crises, all affect the potential for leadership – however defined – to result in beneficial outcomes for society. What we understand less is whether or not political leadership is a culturally contingent concept. If we control for type of institution and level of government, is effective leadership in Porto Alegre, Brazil, the same as effective leadership in a rural village in India? Are these the same as effective leadership in Otavalo, Ecuador? That is, in what ways do different cultures value, and tend to produce, different leadership qualities? To what extent are leadership qualities universal? Can we learn and

adapt successful leadership styles from other cultures?

How do strategic actors use information and causal frameworks?

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Both rational choice and cultural frameworks in comparative politics treat the acquisition of cognitive frames and information as endogenous or uninteresting. Rational choice theorists assume either costless information or an acquisition of information only as bad outcomes warrant an effort up to the point at which costs and benefits of additional information equalize. Iterated games yield the equilibrium state. What does not matter in this perspective, however, are actors' different endowments and techniques to construct cognitive maps and to interpret the nature of games themselves, as differences in such starting points wash out as games unfold.

Cultural theorists typically assume interpretive frameworks that can absorb all incoming information. Even if there are multiple frames, it is unclear how a frame can be challenged by a rival. But what are the conditions and processes that make actors change their cultural frameworks in which they construct causal relations and choose appropriate objectives and strategies? What exogenous experiences might persuade actors to modify or abandon cultural frameworks, or just be unable to recruit new believers?

Comparative politics should invest more time investigating the interplay between exogenous information and experiences and the (re)construction of cognitive frameworks actors

employ to devise strategies to help them arrive at “optimal” pursuits of preferences and values. In many areas, investigators may want to study (1) how actors process data and how they assemble data in causal maps and (2) how this process of constructing data and causal models in political life varies in space and time. Exemplary subjects where these questions can be studied are (1) economic voting (How attribution of responsibility for outcomes?), (2) party competition (How do politicians construct voters’ act of electoral choice?), (3) institutional choice (How do bargaining parties construct the consequences of institutions?) or (4) inter-ethnic relations (construction of collective identities, attribution of interests and strategies to ethnic collectives).

How do ideational interests shape policy preferences and outcomes?

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What is the role of ideational interests – to borrow Max Weber’s term – in shaping policy preferences and, ultimately, policy outcomes? By ideational interests, I mean anything from a desire for an afterlife (religion) to a belief in “proper” gender roles. The research questions that ideational interests pose for comparative politics include: Do ideational interests vary across countries? Do they work in the same way in all countries? Do normative or religious attitudes vary systematically from one country to another? Do they give rise to specific institutions that sustain a society? If so, do these institutions reinforce specific attitudinal orientations? Do individuals with the same religious orientation behave politically the same ways in all countries? Aside from vibrant cot-

tage industries on social capital and identity, comparativists pay relative little attention to the psychology of citizens. Survey-based studies are fewer in number in comparative politics than in American politics. Yet since the World Values Surveys have become available, economists have employed cultural norms both as dependent and independent variables in their large-N cross-national studies (Alesina and Giuliano 2007; Barro and McCleary 2005; McCleary and Barro 2006; Guiso, Sapienza, and Zingales 2006, to cite a few). Economists, however, have not fully answered the questions mentioned above. Comparative politics scholars, who are likely to know several countries in depth, are better suited than economists to understand how institutional and socio-economic variables interact with ideational variables. Recall that it was Robert Putnam’s work on Italy that inspired the current scholarly interest on ideational variables. Comparative politics has had its infatuation with culture in the past. Maybe it is time to take another look at ideational issues. In addition to survey-based research, the recent advancements in the use of experimental methods in social sciences offer new possibilities for unpacking the unexplored mysteries of political man (see Druckman et al. 2006).

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What are the cognitive foundations of individual political decisions?

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Scholars of comparative politics make productive use of an array of paradigms: materialism, institutionalism, and cultural/cognitive analysis, for starters. Although each of us has our favorite corner of the subfield, most of us are eclectic in putting together causal stories with multiple elements depending on what question we’re asking. Gender stereotypes (a cultural/cognitive factor), for example, govern whether girls are socialized to be assertive labor market participants or docile candidates in the marriage market. To explain gender stereotypes, Torben Iversen and I (forthcoming) credit the relative importance of brawn in economic production (a material factor). Female political representation, however, is best explained by an interaction of female labor force participation (material) and electoral rules (an institutional factor).

Establishing “micro-foundations” in the choices of individuals and the aggregation of those choices is a way

to build confidence in causal models. But the truth is that we still know relatively little about the human mind so we are forced to make assumptions about what motivates people. A Big Unanswered Question is how humans really think: how much is hard-wired, how readily humans can switch across different types of logic that reorder priorities, how differently people behave in group settings, and so on. This does not imply that we should all become psychologists; neuroscience is not our disciplinary comparative advantage and cognitive factors are only one set of factors at play. But it does make me optimistic that our knowledge of politics has considerable room to improve as we learn more about who we are.

Gender

Is gender inequality responsible for the prevalence of dictatorship in the Muslim world?

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Many scholars of late have addressed this question quantitatively. Fish's regressions show that Islam is associated with lower values on several indicators of gender inequality, and that these indicators then help to account for variation in levels of political freedom (2002). Donno and Russett rework Fish's models in ways that suggest gender inequality indicators are unrelated to levels of democracy, and also that Islam is not a cause of poor gender outcomes (2004). Inglehart and Norris find Muslims to be less supportive of gender equality than members of other major religious traditions (2003a), and that support for gender equality is

positively correlated with levels of democracy (2003b). Yet Jamal and Langohr find that neither attitudes toward gender nor objective gender indicators matters for democracy.

To move forward on this question, it is time to work on the conceptual front, mobilizing the insights of specialists on gender and politics in the Muslim world. Three of the works mentioned begin with regression models, turning to conceptual matters secondarily and only preliminarily. Why *would* gender relations impact the shape of political systems? What are the causal mechanisms through which these phenomena might be related? Are other factors suppressing both women's rights and democracy in the region? The literature cannot advance much farther – with new and/or better-specified models – until scholars grapple more systematically with such questions. Issues surrounding women's rights and dictatorship frame much of the conflict between the West and the Muslim world, so we must get the answer right.

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Why do men dominate politics?

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When Tony Messina and Michael Coppedge invited me to write a paragraph on a "big unanswered question in comparative politics," two questions came immediately to my mind. First, why are women under-represented in national government everywhere? Why do men dominate politics everywhere? How is it that one sex rules and another doesn't – and hasn't?¹ This question is so major that it has been normalized as invisible in our subfield. Second, how can we, as comparativists, develop a comparative politics of gender? That is, what could we learn by locating gender as a central comparative political concept? And if we answer this second question, will it help us to answer the first?

On its face, a comparative politics of gender may appear to concern only political women, and to have little to do with, for example, the political economy, political development, democratic transformations, or organized political violence. A comparative political analysis that begins with gender will not only position us to answer the question of women's universal political exclusion. Placing gender as central in our research permits us to analyze the political consequences of gender. Do differences in gender rela-

tions explain differences in levels and types of political development? How do gender relations constitute and shape political structures, state institutions, and policy-making processes? Does gender establish path dependencies in institutional development, in democratic inclusion, and in the political organization of violence?

A comparative politics of gender also positions us to analyze the political construction of gender. How do major structural transformations, governmental policies, the political economy, and militarization, for examples, produce gender, and to what ends? Answers to these questions should lead us to deeper (and perhaps more accurate) understandings of the comparative political phenomena that have long been the defining foci of the subfield.

Notes

¹ With the recent exception of Liberia.

Rethinking *The Economic Origins of Dictatorship and Democracy*: The Continuing Value of Cases and Comparisons*

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The publication of Daron Acemoglu's and James Robinson's *Economic Origins of Dictatorship and Democracy* (New York: Cambridge, 2006, hereafter EO) in many ways marked the peak of a trend toward supplanting in method if not in substance the "qualitative classics" in democracy studies. Eschewing 100-page treatments of particular cases in history and the direct comparison of small sets of countries, Acemoglu and Robinson depart from history on about page 46, spending the next 300 pages developing formal models of democratic transition and breakdown. Along the way, they point continually to the shortcomings, causal inadequacies, logical problems, contradictory conclusions, and generally theoretically unsatisfying nature of the "qualitative" or "case study" literature on democratization. They return to historical cases in the conclusion, revisiting their first chapter analysis of Argentina, Britain, Singapore, and South Africa to suggest that the four-country sample provides strong support for the theory. Given the award-sweeping year the authors enjoyed in 2006¹ and the ambitious claims they

make throughout the book, it seems fair to hold it to the highest explanatory and empirical standards. Here I ask whether the authors have in fact moved the field of democracy studies beyond its current standard of "rhetoric and anecdotes,"² and suggest that the answer is no. I do so from a sense of ambitious positivism that I share with the authors: that as social scientists we can, and should, seek general explanations for important outcomes like democracy that apply as broadly as possible. However, I depart fairly radically from the authors' claims that the "mainstream, case study literature" has been so unproductive and that their formal modeling of democratic emergence and breakdown provides a substantial advance in the field. My critique of the book has its origins in some classic lessons on comparative analysis and serves as a useful reminder of the value of "cases" and "case studies." Indeed, it is possible to use the four countries addressed in its first chapter to test the book's main assumptions.

"My critique of the book has its origins in some classic lessons on comparative analysis and serves as a useful reminder of the value of 'cases' and 'case studies.'"

Within-case and cross-case analysis of the four countries Acemoglu and Robinson (EO, 1) cite as "canonical" and "exemplary" illustrations of the argument—Argentina, Britain, Singapore, and South Africa—not only fail to provide support for but instead directly contradict the book's

two central theoretical foundations. In short, workhorse methods in qualitative comparative inquiry illuminate some serious problems with the concepts and models in *Economic Origins* and vindicate the use of historical case studies and structured comparisons for theory testing. In the sections that follow, I summarize the book's argument and introduce the cases used to illustrate its main dynamics. I then use cross-case comparison to test the fit of the theory with the sample of countries used in the opening and closing chapters of the book, and within-case analysis of Singapore to illustrate causal and endogeneity problems in the argument.

The Argument

The basic political ecology used to develop the book's formal models of regime dynamics is one populated by an elite class and a lower class. Both are motivated collectively by their economic interests: elites want fewer taxes and less redistribution, the lower class wants more of both (EO 99-113, esp. 103). To stave off dissent or the threat of revolution under nondemocratic regimes, elites prefer to make moderate redistributive concessions within the context of existing institutions (26, 144-51, 157-58). However, elites face a problem with this strategy: they generally cannot credibly commit to sustaining those policies without wrapping them in democratic institutions that lock in the policies (80, 83, 133-36). The lower classes generally prefer democracy to autocratic policy shifts because, under democratic government, their numbers can ensure a credible commitment to redistribution.

Lower levels of inequality generate no such social pressure; here the masses remain content to live under autocracy because their economic interests have been met (EO,1). The authors

employ the case of Singapore to illustrate this dynamic, and I return to it below. By contrast, when economic inequality is extremely high, as the case of South Africa illustrates, elites see democracy as so threatening that they are willing to fight to prevent it much longer than in other settings. In between are levels of inequality that one reviewer characterized as democracy's "sweet spot" (Galbraith 2006; EO 37). Illustrated by the cases of Argentina and Britain, medium levels of inequality make democratization possible (EO, 244) without a subsequent turn to radical redistribution. As a result, given a middle range of social inequality, elites can prevent social uprising only by conceding to the political representation of the masses. And, under middle levels of inequality elites' positions are not so privileged that they feel they must protect them at any cost by resorting to repression.

"I conduct a preliminary cross-case analysis of Argentina, Britain, Singapore, and South Africa, and a within-case analysis of Singapore to assess the explanatory purchase of Acemoglu's and Robinson's argument."

In short, relative levels of inequality (both across countries and within countries over time) largely determine the likelihood that elites will accede to democracy. Moreover, only democratic institutions can provide the credible commitment to redistributive policies

demanded by the lower class. But do these propositions track with the actual political histories of these countries? Below, I conduct a preliminary cross-case analysis of Argentina, Britain, Singapore, and South Africa, and a within-case analysis of Singapore to assess the explanatory purchase of Acemoglu's and Robinson's argument.

Cross-Case Comparisons: Inequality and Democratic Transition

Does variation in inequality correlate with variation in the emergence of democracy in these cases? The most equal and most unequal cases in a sample should all evince no democratization, or only democratization after a protracted struggle in the latter case. Those in the middle range are more likely to democratize. Table 1 presents Gini coefficient data for Argentina, Britain, Singapore, and South Africa for either the year before democratic transition or, when data are unavailable, for the closest year to that date.³ Gini coefficients are the most commonly employed inequality data in EO despite the authors' qualification of their match to inequality (EO 35-36), and are the most widely available for cross-national analysis so it seems fair to stipulate to the authors' preferred measure. For the sake of comparison with similar cases, I included data for Botswana, Chile, Hong Kong, and Spain to pair with South Africa, Argentina, Singapore, and Britain, respectively.

We should expect countries with the greatest equality (lowest Gini coefficients) to be least likely to democratize. The reason is that, according to EO, citizens essentially act economically; when social equality is relatively high, lower classes do not demand democracy. Singapore, the book suggests, illustrates this dynamic canonically. Why, then, did a democratic transition take place in Argentina in

Table 1: Comparative Inequality and Democratic Transitions*

<u>Country</u>	<u>Year</u>	<u>GINI</u>	<u>Outcome in Following Year</u>
Argentina	1973	33.40	Democratic Transition
Argentina	1981	42	Democratic Transition
Chile	1973	44	Military Coup
Chile	1989	51.88	Democratic Transition
Britain	1823	44	First Reform Act
Britain	1871	62	Ballot Act (1873)
Britain	1881	55	Corrupt and Illegal Practices Act (1882)
Britain	1991	47	—
Spain	1973	37.11	Democratic Transition
Singapore	1966	50	—
Singapore	1973	41	—
Singapore	1989	39	—
Hong Kong	1991	45	—
Botswana	1986	54.21	Democratic Continuity
South Africa	1960	55	—
South Africa	1993	62.30	Democratic Transition

*With the exception of Britain, all data are from Deininger and Squire 1996. Data for Britain in 1823, 1871 and 1881 are from Williamson 1985, and for 1991 from Bourguignon and Morrison 2002, both cited in Acemoglu and Robinson 2006, 69.

1973 when social inequality was significantly *lower* than in Singapore in the same year? Second, at the highest levels of inequality, we should not expect many democratic transitions except, as illustrated through the case of South Africa, following protracted violent struggle as elites fought fiercely to preserve their disproportionate power *and* after inequality levels fall. Yet we saw a relatively peaceful period preceding the enactment in Britain in 1873 of the Ballot act, at a level of social inequality almost identical to South Africa's.

Third, and following the description in EO of Britain's gradual redistributive trend, democracies should over time produce lower inequality through redistribution. So, why was Botswana, Africa's longest-lasting democracy, as unequal in 1986 as South Africa was 26 years earlier? Fourth, democratic transitions took place at all levels of inequality represented by these data and democracy appears to have con-

solidated following transitions in polities with a wide array of inequality levels, with Gini coefficients ranging from 37 (Spain 1973) to 62 (Britain 1873, South Africa 1994). And, in South Africa, where the authors suggest that "democratization [was] caused by falling inequality... in the 1980s," a somewhat longer historical look reveals the opposite: that between 1960 and 1993 inequality actually rose. Finally, a glance at the range of Gini coefficients across these cases in the context of worldwide averages reveals that even the "most unequal" countries in this sample compare pretty well globally. As Sutcliffe (2007, 5) notes, "It is ironical that the world is more unequal than South Africa, whose inequality once aroused the almost universal fury of the world."

It is unclear from these figures whether comparative inequality bears any relation to the likelihood of democratization. It is possible, of

course, that the additional factors developed in subsequent sections of the book—globalization and asset mobility, and civil society for example—might account for all of this variation. Outside of brief comments in the conclusion, however, none of these additional factors are given serious attention in the presentation of the cases, so it is difficult to ascertain how they influenced the outcomes or how important they are relative to inequality. And, in the main discussion of the effects of the array of variables (EO 31-46), inequality takes pride of place, suggesting that it is both the most important factor and a fairly deterministic one (see also 192-93). In any case, a preliminary analysis of EO's four exemplars casts doubt on the inequality-democracy nexus. More importantly, it reminds us of the value of preliminary small-N comparisons across cases, also referred to as plausibility probes. Such comparisons can send us fruitfully into greater depth in particular cases. I turn to one, Singapore, in the next section.

Within-Case Analysis: Testing Logics and Causal Mechanisms

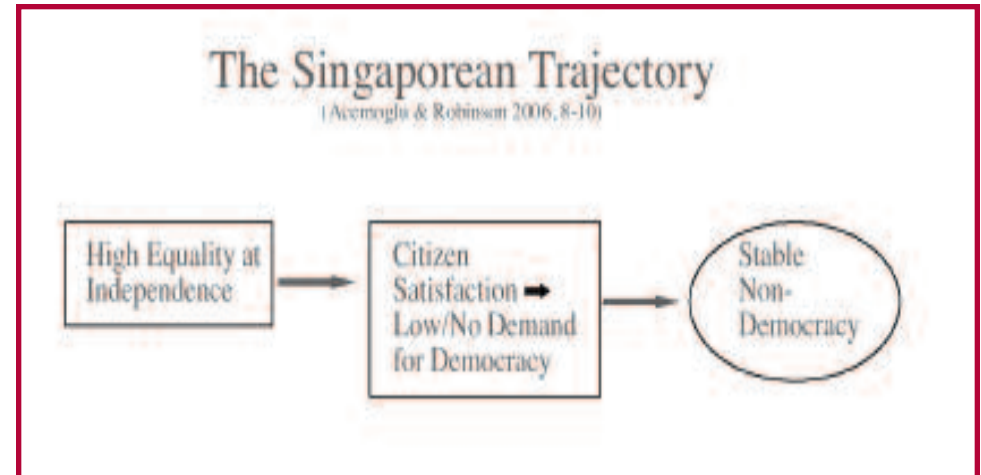
My comparison of the cases above provided an initial check on the relationship between inequality and democracy in comparative static terms. What my comparison did not do is explore any of the mechanisms linking the two. As Acemoglu and Robinson rightly suggest (85-87), no account of the emergence of democracy, as catalyzed by inequality or anything else, is very satisfying without explicit attention to the theoretical development and testing of causal logics. On this point I concur entirely with the authors.

The authors contend, reasonably, that rulers and elites must provide a credible commitment to the lower class in order to address successfully the

threat of rebellion. In such a scenario, autocrats who promise redistribution within the political status quo cannot gain the trust of the lower class. As a result, rebellion becomes likely. A second key contention is that, as the authors suggest Singapore indicates, relatively egalitarian societies may never have to democratize in the first place because “people are sufficiently satisfied under the existing political institutions” (1, 144-51). They imply a causal path, or trajectory, of Singaporean politics from independence to the present, in which nature mercifully endowed the first post-independence government with low levels of inequality. Figure 1 illustrates this path.

Acemoglu and Robinson (8-10, 192) argue that inequality has been low in Singapore since independence, inferring from a reliable data point for 1973 in the Gini dataset compiled by Deininger and Squire (1996). However, by reference to Singapore data in Table 1 we can observe that inequality declined rapidly between 1966 and 1973.⁴ We also know that politics in Singapore continues to be nondemocratic. The reason that is so, I suggest below, is not because of low inequality across time but because the regime responded in its early years to lower class unrest by adopting aggressive redistribution policies to alleviate inequality. It did so very credibly, not through democratizing but by co-opting most of the lower class into the ruling People’s Action Party. That is, some autocratic institutions provide credible commitments to redistribution.

In British Malaya, as modern-day Malaysia and Singapore were called prior to independence, the Japanese invasion during World War II catalyzed an armed resistance movement in Singapore that carried its strategies of violent political contention over into the post-war British

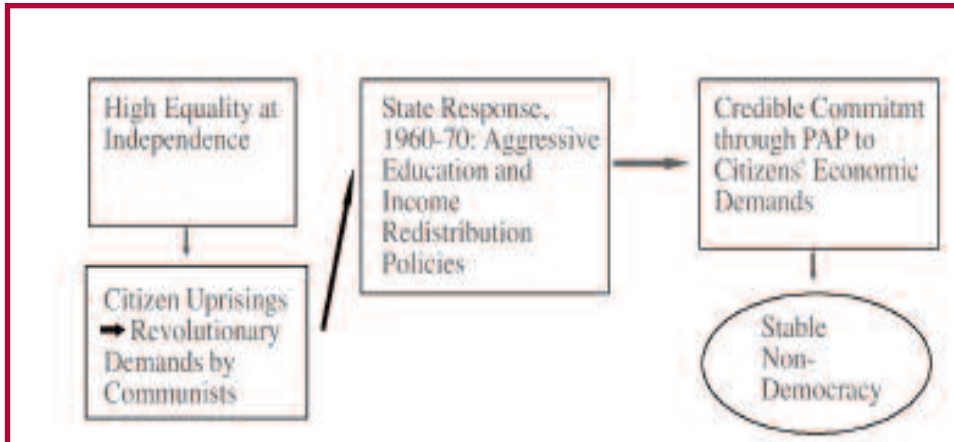


reimposition. This trend peaked in 1947 and continued into the 1950s in the form of strikes and violent riots. In response, the British colonial administration imposed democratic elections on Malaya, accomplishing what revolutionary uprising had failed to—making Singaporean political elites hold democratic elections. The People’s Action Party won an outright majority in the first elections of 1959 on the back of labor support (Doner et al 2005, 347). However, during the early 1960s the PAP continued to be politically threatened by the Socialist Front; it responded *both* by repressing labor and by offering social policies aimed at redressing the Socialists’ demands. The Housing Development Board subsidized land, home construction materials, and financing (Rodan 1989, 73-75). Education was expanded in scope and quality, especially with regard to technical training. The Central Provident Fund, a fund to which firms were required to make payments (i.e. taxing the rich elite), then made the funds available to the working class for housing, education, and health (Rodan 1989, 95). All of these policies served to lessen the level of social inequality in Singapore at the same time they reduced wage pressures, producing a positive-sum outcome for both labor and capital.

Clearly, Singapore didn’t democratize during this period; in response to increasingly radical demands for redistribution, the PAP redistributed wealth to the lower class. And, so far, it has paid off. Moreover, Singapore’s economic elite possesses highly mobile assets—there is no remaining agricultural class—and operates in a highly globalized economy, both of which should augur well for the emergence of democracy yet fail to. I suggest that this has a powerful implication that Jason Brownlee (2007), Dan Slater (2005) and I (2005) have all demonstrated via comparative historical analysis: authoritarian ruling parties can often provide a credible commitment to social actors sufficient to retain their support for long periods of time.

In short, Figure 2 illustrates the actual trajectory of regime dynamics in Singapore. While the first two steps in the progression of regime-lower class relations are identical to the ones developed in EO (176), the result was quite different: nondemocratic institutions provided a long-term credible commitment to redistribution. Of course, the regime employed not only redistribution (which both ameliorated short-term tensions and provided a longer-term solution to permissive social conditions for mobilization); it

Figure 2



also employed coercion, but of a much lesser scope and magnitude than did regimes in countries such as Argentina and South Africa. But what is crucial in this case are the facts that a) authoritarian institutions provided a durable and credible commitment to the lower classes in Singapore and b) it was politics that catalyzed change in the level of inequality rather than the other way around.

Conclusion

Scholars of comparative politics who employ qualitative comparisons of cases have developed over the last 15 years or so increasingly sophisticated standards for selecting their cases and for situating them within larger universes of cases. Using those strategies to situate the four main cases in EO in relation to one another reveals a substantial lack of fit between the theory developed in the book and the dynamics of political regimes in Argentina, Britain, Singapore, and South Africa. And it serves to remind us how useful small-N comparisons continue to be in the quest for ever stronger theories (models) of democratization and democratic consolidation. As a brief within-case analysis of Singapore reveals, case studies can also provide what com-

parative statics cannot: tests of causal sequences and illumination of endogeneity problems in comparative statics models. That is, where the authors argue that equality produced politics, close attention to the historical causal reality of Singapore reveals the opposite: authoritarian politics led to equality.⁵ Moreover, the narrative reveals a history of substantial lower class unrest prior to government policy response, the response itself, and an apparently credible commitment to redistribution that has led to durable autocracy. And, while the regime in Singapore remains outright authoritarian, it is less repressive than many dictatorships, illustrating a political equilibrium that defies the logic of the argument in EO.

These findings give me continued enthusiasm for the use of case studies and structured qualitative comparisons in the search for general explanations of “big” historical phenomena, and they serve as a useful reminder that the most rigorous theoretical exercises are ultimately subject to empirical verification or disconfirmation. By calling into question the central propositions in *Economic Origins*, the cases of Argentina, Britain, Singapore, and South Africa also direct us to fruitful and established standards of theory building and test-

ing that have been with us since the “qualitative canon” began to emerge with works by Moore, Dahl, and others.

Notes

¹ The book won the Woodrow Wilson Foundation and William Riker awards of the American Political Science Association and the Outstanding Book Award in Economics and Finance from the Association of American Publishers.

² So termed by Romain Wacziarg in *Science*, 15 September 2006, 1577.

³ Since Britain’s transition took place well before the Gini data compiled by Deininger and Squire (1996) begin, I use data compiled by Acemoglu and Robinson 2006, 69.

⁴ Unlike the 1973 data point, the 1966 data point is not one of Deininger and Squire’s highest reliability “accept” data pool so it is not possible to be completely confident about the change. However, the change over this decade implied by the two data points squares very well with the scholarly consensus on how the PAP regime reduced inequality by building a safety net for Singapore’s poor.

⁵ Thanks to Dan Slater for making the point to me in this way.

*Thanks to Jason Brownlee, Michael Coppedge, Cynthia Horne, Anthony Messina, Bryon Moraski, Dan Slater, Daniel Ziblatt, and participants at the “Revisiting the Origins of Democracy: Do We Still Need the Qualitative Classics?” roundtable at the 2007 APSA meeting and the University of Florida Research Seminar in Politics, September 17, 2007, for thoughtful comments on previous versions of this essay.

Review of Ethno-Linguistic Fractionalization Data Sets

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Over the past few years, measures of ethnic fractionalization have been increasingly employed as a control variable in comparative politics. Fractionalization has been used to explain variation in levels of cross-national economic growth, corruption, levels of democracy, quality of governance, provision of public goods and conflict propensity (Easterly and Levine 1997, La Porta et al. 1999). Agreeing on a standard index of ethnic fractionalization, therefore, ought to be a pressing research project for the subfield. This article reviews the current state of the data on ethnic fractionalization and recommends a more concerted effort to create a long-term ethno-linguistic fractionalization dataset.

While ethnic fractionalization is increasingly used as an explanatory and control variable in quantitative analyses, ethnicity as a theoretical concept has never been more contested. Despite the popularity of a few datasets, such as those compiled by Fearon (2003) and Alesina, Devleeschauer, Easterly, Kurlat and Wacziarg (2003), scholars continue to wonder how exact “ethnicity” can be defined as a theoretical concept, and whether it should even be used in our theoretical constructs at all (Chandra 2006). Even the authors of two of the best collections of ethnic datasets

reviewed here cast theoretical doubts on their ethnic constructs. Alesina et al. (2003) recognize ethnicity as a “vague and amorphous concept” and Fearon (2003) writes that it is “inherently slippery.” There are several fundamental quandaries which must be addressed in the construction of a dataset on ethnicity, and their resolution depends in large part on what one thinks “ethnicity” as a variable is capturing. The ethnic fractionalization datasets under review each respond to the basic quandaries by measuring ethnicity in slightly different ways, producing slightly different fractionalization indices, which do not always measure up.

“While ethnic fractionalization is increasingly used as an explanatory and control variable in quantitative analyses, ethnicity as a theoretical concept has never been more contested.”

The first and most fundamental difficulty in constructing a dataset on ethnicity is deciding who counts as an ethnic group. There are several reasonably objective attributes which can be used to distinguish one ethnic group from another in a consistent way, such as language, race, religion, tribe, descent, nationality, or some combination of them. While one method, discussed below, is to disaggregate ethnic fractionalization indices into different attributes, the classic method is to create one ethnic fractionalization measure which differentiates ethnic groups by the “home” languages that they speak, the assumption being that language ties

are the most salient feature that sets ethnic groups apart from one another.

“The first and most fundamental difficulty in constructing a dataset on ethnicity is deciding who counts as an ethnic group.”

If this is the agreed-upon attribute which best distinguishes ethnicities, then it would be natural for scholars to use the ethno-linguistic fractionalization index which does the best job of counting up the world’s languages, such as the *Ethnologue* index, arguably the most comprehensive and fine-grained catalogue of languages spoken in the world. The *Ethnologue* is compiled by anthropologists, linguists and geologists and lists over 6,800 distinct languages. However, no political scientist uses the *Ethnologue*’s listing by itself as a measure of ethnic fractionalization. The case of Papua New Guinea illustrates why. By the *Ethnologue*’s count, Papua New Guinea is the most ethno-linguistically diverse country in the world, with the greatest density of distinct languages. The problem is that the fractionalization in this country has little relevance for politics. While some scholars disagree with the choice (see Reilly 2000), most political scientists are only interested in ethnic groups that are politically meaningful. For example, one could classify citizens of the United States by family names, counting up the numbers of Smiths, Jones, Johnsons, and Browns, however this would not tell us much about political behavior. So it is with language groups in Papua New Guinea, which are so thoroughly disaggregated that it is

nearly unthinkable that they would ever provide a basis for political mobilization.

“ [...] most political scientists are only interested in ethnic groups that are politically meaningful.”

The difficulty, then, is in choosing which language groups matter. For a first cut, Fearon (2003) chooses to count only ethno-linguistic groups that make up more than one percent of their respective national populations. Then, to deal with the problem that not all languages are equally distinct, he categorizes these ethno-linguistic groups according to linguistic-cultural distance. Using his scale, a country with a population equally divided between Arabic and English speakers is of a higher fractionalizational order than one similarly divided between French and Italian speakers.

In doing so, Fearon (2003) follows Scarritt and Mozaffar (1999) and Barrett (1982) in categorizing differing levels of ethnic aggregation. Scarritt and Mozaffar's index (1999), for example, offers three levels of ethno-linguistic fractionalization. The first level considers only those countries whose national population is politicized on one side or another of an ethno-political dichotomy, such as the Hutu-Tutsi divide in Rwanda. Their second, “middle” level of aggregation lists ethno-political groups and coalitions who work together politically but do not polarize into a national dichotomy, and their third, “lower” level further disaggregates these middle level ethno-coalitions if they contain “significant ethno-political cleavages.” Barrett (1982) offers 11 progressively detailed levels of aggregation, which, like a biological classifica-

tion, starts with 5 races, splits into several skin colors, geographical races and sub-races, and is then followed by ethno-linguistic families (71), peoples (432), constituent peoples (8,990), languages/sub-peoples (7,010), dialects (17,000) and so on.

As with many of databases, these illustrate typical problems of transparency and objectivity in coding which ethno-linguistic groups count and at what level. Barrett and his team, for example, were also compiling the *World Christian Encyclopedia*,¹ for which they counted languages as distinct only if those languages “need a separate bible translation” for evangelization purposes. It is not clear whether the need for bible translations faithfully reflects the inherent distinctiveness of language communities or something else, such as evangelization budgets, missions or administrative districts. The decision to categorize an ethno-linguistic community as a full “language” as opposed to a “dialect” also highlights the point that while Barrett (1982) and other indices like his are impressively thorough, many subjective decisions are made when assigning people to their various categories. One has to be skeptical about some of these numbers and the data analyses that use them.

“[...] these [databases] illustrate typical problems of transparency and objectivity in coding which ethno-linguistic groups count and at what level.”

As Barrett's (1982) classification implies, ethnicity not only refers to language, but to race, color, and often

religion. While in places like Africa language seems to be the best way of dividing peoples into categories, in the United States or South America, most people speak the same language even though they may simultaneously belong to many different racial and religious categories. Some authors, therefore, introduce separate race and religion categories to their ethno-linguistic indices. Alesina et al. (2003), for example, include racial and religious indices, and Annett's (2001) index also includes a religious subcategory alongside ethno-linguistic ones.

An alternative to counting the ethnic groups that are potentially relevant politically is to count only those ethnic groups that are actively relevant. Thus, instead of distinguishing between race, religion, or language, or attempting to make rough decisions based on anthropological categories, or counting groups only above a certain threshold, Posner (2004) decides to count only the ethnic groups he decides are politically relevant. His Politically Relevant Ethnic Group (PREG) fractionalization index codes ethnic groups that are currently politically active and mobilized; he calculates the index in two time periods in order to track changes over time. For now, his dataset is available only with respect to sub-Saharan Africa, but it has the potential to be calculated worldwide.

With all these datasets, an additional problem has to do with the sources used. Many of the earlier fractionalization measures, such as Hudson and Taylor (1972), Mueller and Murrell (1986), Easterly and Levine (1997), and La Porta et al (1999), used the infamous *Atlas Nirodov Mira* (1964), a Cold War-era index created by Soviet ethnologists. This index was discredited both because it seemed to misspecify certain ethnic groups (famously making no distinction between Hutu and Tutsi in

Rwanda) and because ethnicity changes over time, in population shares and political significance. As Fearon (2003) points out, while most political science scholars until the early 1990s considered Somalia to be ethnically homogenous, this is no longer the case, as a lower order of ethnicity has increased in salience and Somalis increasingly distinguish themselves more by these more disaggregated categories. While some of the newer datasets, such as Alesina et al. (2003), Fearon (2003), Posner (2004), and Annett (2001), try to overcome this problem, they often end up using the same data sources as a baseline. Both Fearon (2003) and Alesina et al. (2003) use the *Encyclopedia Britannica*, the *CIA World Factbook* and the Ethnologue project. Annett (2001) uses Barrett (1982) who also draws off of the Ethnologue project and the *Encyclopedia Britannica*. Posner (2004) translates and then uses the *Atlas Nirodov Mira* (1964) along with Morrison's (1989) *Black Africa: A Comparative Handbook*, which Fearon (2003) also references. All of these scholars supplement their missing "gaps" with newly available census data. However, despite using similar sources, the fractionalization measures vary significantly. While they are generally close, as Posner (2004) reports for sub-Saharan Africa, Alesina et al. (2003) correlates with Fearon (2003) at 0.73 and both of them correlate with Posner at under 0.54. Overall, Alesina et al. (2003) correlates with Annett at 0.88. While these correlations are not bad, considering that they collectively employ similar sources and methods, the result is somewhat disappointing.

As most of these scholars recognize, ethnic fractionalization measures are still fairly weak. The discrepancies among them underscore the need to have a more standardized and regular counting of ethnic fractionalization measures that can account for differ-

ent kinds of ethnicity, as well as change in ethnicity over time. One could theoretically begin constructing a standardized, multi-dimensional ethnic fractionalization measure using worldwide survey data based on ethnicity, language, tribe, religion and race, as Fearon (2003) suggests. This would allow scholars to classify ethnic groups over time according to how individuals categorize themselves, with varying levels of significance for their multiple identities. There is some current work along these lines. Lind (2007) has recently used opinion polls to classify groups by size and distinctive cultural distance in the United States, while Dowd and Driessen (forthcoming) use Afrobarometer data to measure ethnic fractionalization by levels of ethnic voting in sub-Saharan Africa. Innovative work by Cederman and Girardin (2007) aims to get at differences in kind by introducing a weighted measure in their fractionalization index for the ethnic group in power (in Eurasia and N. Africa). While all are innovative, these pieces still lack cross-national generalizability.

“The discrepancies among [ethnic fractionalization measures] underscore the need to have a more standardized and regular counting [...] that can account for different kinds of ethnicity, as well as change in ethnicity over time.”

For range of data, rigor of method, and ease with which scholars can calculate ethnic fractionalization sev-

eral different ways, Fearon (2003) and Alesina et al. (2003) remain the standard, covering 160 and 190 countries, respectively, with each index measured in at least three different ways. As the authors acknowledge, these datasets should not be the last word on ethnic fractionalization. Until a more systematic attempt is made to regularly measure ethnic fractionalization, however, their indices are a great help to scholars trying to pick apart the relationships between ethnicity and politics.

Notes

¹ The *World Christian Encyclopedia* was updated in a new edition in 2001 and much of its data can be found at <http://worldchristiandatabase.org/wcd/>

² Cederman and Girardin (2007) also attempt to overcome the shortcoming of the Herfindahl probability index, Ethno-linguistic Fractionalization =

$$1 - \sum_{k=1}^K p_k^2$$

which all these indices use. The index measures the probability that two randomly selected individuals from the entire population will be from different ethnic groups. However, as Fearon (2003) points out, the Herfindahl index cannot distinguish differences in kind of national ethnic structure. In nearly identically fractionalized countries of 0.75, one country could have four ethnic groups of equal size and the other have one large ethnic group at 48% of the population and many smaller ones at 0.01%. Since at least Horowitz (1985) on, scholars have suspected that these two different national ethnic configurations have vastly different implications for national political outcomes.

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Dataset Announcements Gapminder World

The Gapminder Foundation has assembled eighteen socioeconomic time-series for varying numbers of countries, and various spans of years, into the Trendalyzer software package. Google has made this package available for limited use on the Internet at www.gapminder.org/world. The software creates animated scatterplots that bring global trends to life. Any of the variables can be placed on either axis or used to specify the size of the countries in a bubble plot. Countries are color-coded by region, and individual countries can be selected for "trails" that show their path of change over time as the animation proceeds. The package can also produce color-coded maps.

Global Integrity Index

A Washington-based organization, Global Integrity, has produced 23 indicators of Civil Society, Public Information, and Media; Elections; Government Accountability; Administration and Civil Service; Oversight and Regulation; and Anti-Corruption and Rule of Law. Its ratings cover 26 countries in 2004, 42 countries in 2006, and 48 countries in 2007. In Global Integrity's own words; "The Global Integrity Index assesses the existence, effectiveness, and citizen access to key national-level anti-corruption mechanisms used to hold governments accountable. The Index does not measure corruption. Rather than examine the 'cancer' of corruption, the Index investigates the 'medi-

...being used against it – in the form of government accountability, transparency, and citizen oversight. The Global Integrity Index is generated by aggregating more than 300 Integrity Indicators systematically gathered for each country covered. For the Global Integrity Index: 2007, those indicators comprised more than 15,000 peer-reviewed questions and answers scored by in-country experts in the summer of 2007. Several rounds of review are conducted at the international level to ensure that cross-country comparisons are valid. In addition, all assessments are reviewed by a country-specific, double-blind peer review panel comprising additional local and international subject matter experts.”

Source:

<http://report.globalintegrity.org/globalindex.cfm>

DataGob

The State, Governance, and Civil Society Division of the InterAmerican Development Bank's Sustainable Development Department has launched a web data portal with financing from the British agency DFID (Department for International Development). DataGob allows users to see averages, create line and bar charts, and download Excel spreadsheets of 400 indicators, drawn from thirty different sources, which include academic institutions, non-governmental organizations, private firms and multilateral agencies. The database includes indicators of governance, democracy, rule of law, decentralization, markets, society, public administration, and transparency. There are also links to information on sources, methods, reliability, and validity. All information is presented in both English and Spanish. DataGob can be found at

<http://www.iadb.org/datagob/>.

Psephos

Adam Carr, an Australian journalist, activist, and elections enthusiast, claims that his elections archive is “the largest, most comprehensive and most up-to-date archive of electoral information in the world, with election statistics from 175 countries.” He is probably correct. This archive reports national votes (absolute and percentages) by party or candidate in presidential (where applicable) and legislative elections for the most recent pair of elections in practically every country in the world. It also reports votes and seats in legislative elections, and it has dozens of election results maps showing election districts. Carr has also written a brief political history of each country that often includes some information about the major political parties. The site appears to be updated constantly. Link:

<http://psephos.adam-carr.net/>

Contestation and Inclusiveness

Michael Coppedge, Angel Alvarez, and Claudia Maldonado have produced indicators of Robert Dahl's two dimensions of polyarchy – contestation and inclusiveness – for nearly all countries from 1950 to 2000. The indicators are the by-product of a principal components analysis of the most extensive and commonly used indicators of democracy. The analysis, which is forthcoming in the July 2008 issue of the *Journal of Politics*, claims that about 75 percent of what Polity, Freedom House, ACLP/Cheibub and Gandhi, and other indicators have been measuring is variation on Dahl's two dimensions. The complete dataset is available at <http://www.nd.edu/~mcoppedg/crd/datacr.htm>.

Editors' Notes

The editors welcome suggestions of other relatively new and potentially useful datasets that should be announced or reviewed in *APSA-CP*. Anyone interested in reviewing a dataset for the newsletter should contact Michael Coppedge at coppedge.1@nd.edu.

We invite our readers to request hard copies of back issues (beginning with the winter 2003 newsletter issue) at the cost of \$1.50 per issue. They should send their request(s) by email to kschuenk@nd.edu.

Announcements

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March 1, 2008

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Nomination Deadline
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Sage Best Paper Award
Award Deadline
March 1, 2008

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Award Announcement

The American Political Science Association's Organized Section on Politics and History is pleased to announce the establishment of a new prize, for the "Best Dissertation in the Field of Politics and History." The first such award will be presented at the section's business meeting at the 2008 APSA convention. We welcome nominations of outstanding dissertations from Ph.D.s awarded in either 2006 or 2007. To nominate a dissertation for this award, please send an abstract of the dissertation and a supportive letter from the advisor or other faculty member of the dissertation committee to the section's Secretary/Treasurer, David Robertson at daverobertson@umsl.edu.

The deadline for initial nominations is March 1, 2008. The awards committee (Theda Skocpol (chair, Harvard University), Paul Frymer (University of California, Santa Cruz) and Sheri Berman (Barnard College), will subsequently be in touch with advisors for full copies of dissertations selected from among this initial pool of nominations.

Call for Papers

Equal Opportunities International Conference

Preliminary Information, January 2008

Primary contact for queries: Ruth Bridgstock
Research Fellow, DECER
[e r.bridgstock@uea.ac.uk](mailto:r.bridgstock@uea.ac.uk)

Stream 2: Multiple Discrimination and Mapping Diversity*
Co-chairs: Iyiola Solanke, University

of East Anglia and Jennifer Hochschild, Harvard University.

The Equal Opportunities International conference will be from 1-3 July, 2008, at the University of East Anglia, Norwich. The conference will provide an international platform for exchange of knowledge across all strands of equality, diversity and inclusion at work. The conference will have a total of about 80 delegates and nine streams, in which there will be approximately eight presentations each. In addition, there will be several high profile keynote speakers, and a number of professional development sessions.

The call for papers will open early in February (at <http://eoi-conference.org/>), and will close on April 11th, 2008. Papers will be selected by May 1, and final versions of papers are to be sent to the EOI Conference organiser for online publication on June 6th. Conceptual and empirical papers relating to the conference theme and Stream guidelines are welcome. These papers are to be no more than 5,000 words in length. Paper presentations at the conference will be a maximum of 20 minutes long, with 10 minutes for questions and discussion. Data projectors will be available in each conference room.

Registration Fees

Registration fees for the conference will be £400. This price includes:

- full attendance at the conference, 1-3 July 2008
- accommodation for the nights of 1, 2 July 2008 at UEA Guest Suites
- all meals and refreshments for the conference, including the welcome function at the Sainsbury Centre, the conference dinner at the Cathedral, and either

a cruise on the Norfolk Broads or a tour of Norwich Castle.

Travel expenses are not included in the registration fee. Registration will be available online through UEA Conferences.

Journal Comparative Sociology Seeking Submissions

Comparative Sociology is a quarterly international scholarly journal published by Brill of Leiden, Netherlands dedicated to advancing comparative sociological analyses of societies and cultures, institutions and organizations, groups and collectivities, networks and interactions. Two issues every year are devoted to "special topics," and three topics currently open for submissions are: Democracy and Professions; Rule of Law and *Rechtstaat*; and Typologies of Democracy and non-Democracy. Consult the Brill Website for descriptions of each topic www.brill.nl/coso. Editor-in-Chief is David Sciulli, Professor of Sociology, Texas A&M University, and Columbia University Ph.D. in Political Science. Submissions are welcome electronically by e-mail insert at compsoc@tamu.edu not only from sociologists but also political scientists, legal scholars, economists, anthropologists and others. Indeed, the journal is particularly keen to receive works of comparative political sociology and comparative legal sociology. All submissions are peer-reviewed and (initial) decisions are typically made within less than three months.

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Winter 2008

How to Subscribe

Subscription to the APSA-CP Newsletter is a benefit to members of the Organized Section in Comparative Politics of the American Political Science Association. To join the section, check the appropriate box when joining the APSA or renewing your Association membership. Section dues are currently \$8 annually, with a \$2 surcharge for foreign addresses. The printing and mailing of the Newsletter is paid for out of member dues. To join APSA, contact:

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