

# apsa-cp

Newsletter of the Organized Section in  
Comparative Politics of the  
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## Letter from the President

### Does Comparative Politics Need a TOE (Theory of Everything)?

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The central theoretical debate in contemporary comparative politics is usually cast as an argument between advocates and critics of the rational choice paradigm. I prefer to describe the debate as a debate between those who would like to unify the field with a common theoretical framework versus those who think the search for a unified theory to be a waste of time or worse. We are all theorists in the sense that each of us hopes that our preferred explanation of A turns out to explain B and C as well. But we are not all believers in the usefulness of adopting an overarching theoretical framework that would provide powerful deductive tools while imposing hard constraints on the range of acceptable explanations.

The only theory in comparative politics today that is sufficiently powerful and general to be a serious contender for the unified theory is rational choice, which is why the advocates of developing a unified theory tend to be proponents of rational choice. If one accepts the desirability of a unified theory, no criticism of rational choice can be persuasive until an alternative with equal generality and deductive power is available. You can't beat something with nothing, as the saying goes. Nevertheless, it is important not to conflate the debate over the merits and demerits of rational choice with the debate over the importance of a unified theory. Twenty-five years ago, Marxism was a more popular candidate for the unified theory than rational choice. Looking ahead, I would not bet the farm that the leading contender

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for the unified theory in comparative politics will still be rational choice in the year 2025.

The obvious answer to the question "Does comparative politics need a unified theory?" is no. Contemporary physicists struggle with the uncomfortable fact that the theory of relativity is incompatible with the theory of quantum mechanics. Sociology has not had a theory of everything with widespread appeal since the discipline came to the realization that trying to understand Talcott Parsons was not worth the effort. While physicists view the lack of a unified theory as deeply unsatisfactory, sociologists seem unperturbed by their theoretical pluralism.

Not all disciplines lack a unified theory, however. Consider the field of economics. There was a time, roughly from the end of WWII to the mid 1970s, when microeconomic theory, in which prices and wages were assumed to adjust to market-clearing levels, was contradicted by macroeconomic theory, in which prices and wages were assumed to fail to adjust without government intervention. The decline of the Keynesian paradigm, however, marked the end of the micro-macro divide. While the efficacy of Keynesian policies is still vigorously debated, the field of macroeconomics has been rebuilt on microeconomic foundations. Almost all economists today accept the same set of fundamental assumptions. Those who admire economics are impressed by the cumulation of knowledge that the adoption of a common theory allows. Critics of economics are more impressed by the narrowness of the research that results.

Would comparative politics be better off with a common unified theory? The argument in favor of a unified theory goes as follows: Logic is an extraordinarily powerful tool of discovery and explanation. Indeed, one could hardly be a social scientist without accepting the fundamental importance of logical consistency. To state "x" in context A and "not x" in context B should cause discomfort until we have a deeper theory that explains why the two statements are not contradictory. Even when multiple theories are not logically inconsistent, a single theory that can explain a broad range of phenomena inspires more confidence than a theory that has explanatory power only within a restricted domain. The argument on the other side is that a unified theory is not attainable and the attempt to obtain the unattainable represents, at best, a waste of intellectual resources. What is worse, argue the critics, is that allegiance to a common theory induces scholars to dismiss evidence that is not easily explained and to ignore questions that are not easily asked within the theory's assumptions.

Taste seems to play a larger role than philosophical principles in deciding which side we take in this debate. Some of us are attracted to the goal of developing a general theory that would enable comparativists to harness the power of deductive thought. Others of us are more interested in explaining particular events or comparisons. On the one hand, those who are studying a particular event do well to use whatever explanation works best without regard to the elegance or generality of the explanation. There is no metaphysical law that says that the best explanation is the simplest or most general explanation. On the other hand, those of us who are trying to build a deductive theory often bene-

*(Continued on page 31)*

# News & Notes

## Change of Editor of the Newsletter

This is the first issue of the newsletter since the summer of 1995 not edited by Miriam Golden. At its annual meeting in September, the executive committee adopted a set of guidelines for selecting a new editor of the newsletter proposed by an ad-hoc committee consisting of Jim Caporaso (chair), Robert Kaufman and Karen Remmer. With a selection mechanism in place, Miriam Golden resigned as editor. Fortunately, Dan Treisman, who has served as associate editor for the past year, agreed to take Miriam's place until a new editor is selected. We all owe a great debt of gratitude to Miriam for her labors as editor for the past five years and we thank Dan for agreeing to take her place as editor on an interim basis. Anyone who is interested in becoming the next editor should refer to the Call for Bids to Edit the Newsletter on the following page for details.

## Selection of Officers of the CP Section

Members of the section have asked me to explain how the officers of the section are chosen. The executive committee of the Comparative Politics section consists of the president, vice-president, treasurer, editor of the newsletter, APSA program coordinator, four additional at-large members and the presidents of the section during the previous ten years. All positions have two-year terms, except for the treasurer and editor of the newsletter and APSA program coordinator. The process of filling positions begins with a five person nominating committee, appointed by the president. The nominating committee nominates candidates for all positions except for that of president, newsletter editor and the APSA program coordinator. The vice-president is the president elect. Thus, Evelyne Huber, the current vice-president, will automatically replace me when my term ends in six months. The new procedure for selecting the editor of the newsletter is described in the call for bids. The president of the section chooses the APSA program coordinator, a one-year position. In addition, any five members of the section may nominate candidates for any office except president at the annual business meeting or by petition sent to the president prior to the meeting. Offices that are contested are filled by secret ballot at the annual business meeting. The winner is the candidate who receives the plurality of votes.

Those of you who attended the business meeting last September may wonder why no new officers were presented. This year, Nancy Bermeo (Department of Politics, Princeton University) and Robert Jackman (Department of Political Science, University of California, Davis) have replaced Ian McAllister and Jennifer Widner as at-large members of the executive committee. Unfortunately, I did not receive the nominations of Nancy Bermeo and Robert Jackman in time to present them at the annual business meeting. Next year, we will choose a new vice-president, to replace Evelyne Huber, and two at-large members of the executive committee to replace Susan Pharr, Kathryn Firmin-Sellers. The nominations of the nominating committee will be advertised in the summer edition of the newsletter.

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### Call for Bids to Edit Newsletter

The newsletter needs a new editor. All who are interested in becoming the next editor of the newsletter of the Comparative Politics Section of the APSA are encouraged to submit a bid. At the last meeting of the executive committee, the following guidelines were adopted.

The editor of the newsletter will henceforth be a four year term. The next four year term will begin in the fall of 2002. The deadline for submitting a bid is Dec. 31, 2001. Bids should be sent to, Evelyn Huber, (University of North Carolina, Department of Political Science, Chapel Hill, NC 27599-3265, email: ehuber@unc.edu).

A three-person committee, to be appointed by the president of the Section, will select the winning bid. The selection committee's decision will be announced by April 1, 2002. While it is desirable for the Newsletter to locate in different universities, the incumbent editor or editorial team may submit a bid to continue to edit the newsletter for a second term.

The selection committee will use the following criteria to evaluate the bids:

a. Bidding institutions should have a comparative politics faculty sufficiently large to support an editor, an associate editor, and have a pool of possible replacements. Responsibilities of the editorial team include identifying and developing themes, contacting potential contributors, selecting and editing submissions, and overall oversight of the production and mailing process. The editor and associate editor must be able to commit an estimated working time of 2-3 weeks per issue, spread out over a longer period of time.

- b. Bidding institutions should have a pool from which to choose an assistant editor. Estimated time spent by the assistant editor is four weeks per issue. The assistant editor is expected to handle layouts, convert email submissions, arrange for printing and production, and manage a web site. Compensation for this position comes from the bidding institution.
- c. The bidding institution should provide office space, computer equipment, copying, and phone support. Released time for faculty will also be taken into account but is not a requirement. Proposals should include a prospective budget and a statement of administrative support.
- d. Themes, directions, special topics and other ideas of the bidding editors will be taken into account.
- e. Section dues will pay for production and mailing expenses. The bidding university should be able to cover other expenses listed in (c).

### Call for Experts

Would you like to be a pundit? In the past months, the APSA has received a torrent of press queries, mostly about Florida politics or US election law. In response, the APSA is considering the establishment of a directory of names covering all subfields of the discipline. If you are interested in being included in such a directory, you should send your name, full contact information including email address, plus a fairly detailed account of your area of expertise to Sue Davis. You can contact Sue Davis by telephone at (202) 483-2512 or by email at sdavis@apsanet.org.

### Luebbert Award for Best Book in Comparative Politics

This year's committee to choose the best book in comparative politics published in 1999-2000 consists of Ben Schneider, chair (Northwestern University, Department of Political Science, 601 University Place, Evanston, IL 60208-1006, email: brs@northwestern.edu), Roger Petersen, University of Chicago, Program on International Security Policy, 915 East 54th Street, Apt. 1, Chicago, IL 60615, email: rpetersenmo@earthlink.com) and Pradeep Chhibber (University of California, Berkeley, Department of Political Science, 210 Barrows Hall, Berkeley, CA 94720-1950, email: chhibber@socrates.berkeley.edu). Send your nominations to the committee.

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## Laboratory In Comparative Ethnic Processes

The Laboratory in Comparative Ethnic Processes (LICEP) invites applications to fill three open slots. LICEP is an inter-university research group that seeks to improve our understanding of the causes and consequences of ethnic mobilizations by developing and applying improved methodological techniques to the study of such mobilizations. We meet twice a year on an on-going basis to discuss research in progress. The substantive questions we ask and the methods we employ are various: but we have an explicit concern with selecting the method most appropriate to the question that we are discussing and refining these methods through sustained collaboration. Membership in LICEP entails an obligation to participate regularly at our biannual meetings (expenses on travel and accommodation are covered). For more information on LICEP, please consult our website at [www.duke.edu/web/licep](http://www.duke.edu/web/licep). Advanced graduate students and faculty working on comparative ethnic processes are encouraged to apply. Although most current LICEP members are comparativists, we particularly encourage those working on ethnic processes in other subfields to apply. The application should include a c. v. and a 2-page proposal describing a presentation of your work that you believe would be an appropriate for a LICEP meeting. In the course of that proposal, you should explain the larger project for which this particular submission is a part. You should also send an example of current research (book or dissertation chapter, conference paper etc). The deadline is February 15. Applications and inquiries should be sent by e-mail to [kchandra@mit.edu](mailto:kchandra@mit.edu).

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# Letter From the Outgoing Editor

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The job of editing the Newsletter of the Organized Section in Comparative Politics landed rather unexpectedly on my desk when my colleague, Ron Rogowski, asked me take it on. Despite having given it remarkably little thought prior to assuming the job, editing the Newsletter has proved to be perhaps the greatest professional pleasure of this period of my career. I met interesting people, tackled interesting problems, and I hope raised the profile and professional standing of our Section in the discipline. I want to thank above all the three Section Presidents I have worked under -- Robert Bates, David Collier, and Michael Wallerstein -- not only for allowing me to grow in the job but more importantly for encouraging the development of an autonomous and professional Newsletter that could mirror and perhaps even help shape the debate that exists in our subfield. And I want to thank as well the three Assistant Editors -- Terri Givens, David Yamanishi, and now Elizabeth Stein -- who have done the hard work of producing and getting out the Newsletter.

When I came on the job, the Newsletter averaged 23 pages an issue and was sent to some 1,092 members. It now averages 30 pages an issue, and currently is sent to about 1,570 members. This growth in size and membership attests above all to the need we have in as large, as disparate and as fragmented a subfield as comparative politics for an intellectual reference point. I am sure the Newsletter has featured many an article that many have disagreed with; it would not have been sensible (or even possible) to shy away from controversy. But as Editor, I have striven above all to concentrate on issues of importance to comparative politics, to bring to the attention of scholars in our field ideas and works that merit discussion, and to provide a locus for fair and open dialogue so that we all (and especially young scholars) get some idea of where our part of the discipline is going and what some of our finest practitioners actually do. The best work in the comparative field is published in a wide variety of journals, and there is no single outlet that focuses our intellectual attention. This Newsletter has tried to represent the state of our subfield in a fashion that no journal currently achieves.

I asked to step down as Editor when it seemed to me that the Newsletter had effectively tackled many of the intellectual issues that were worrying me. These included the role of replication in comparative research, the intersection of and distinction between international and comparative studies, whether rational choice could properly study culture, and federalism. I used my discretion as Editor to ask intelligent and thoughtful people to comment on what I believed were important substantive issues. But there was no reason to remain Editor once the major issues that concerned me had appeared on these pages; it was time to turn to someone new. I was delighted when my colleague Daniel Treisman agreed to take on the job of Editor. I know that he will bring a new enthusiasm to the job, and have new ideas to propose to the Section officers regarding potential thematic issues. Above all, I trust that our Newsletter will remain fun to read under his stewardship.

Miriam Golden

New York, January 4, 2001

# Symposium

## Cumulative Findings in the Study of Ethnic Politics

Guest Editor

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### **Introduction: Constructivist Findings and Their Non-Incorporation**

Research on ethnic politics, broadly defined, has been piling up for at least a half-century. This symposium asks: What are the cumulative findings generated by this body of research, and to what extent are they, and should they be, informing new research programmes?

This essay argues that the constructivist approach to ethnic groups has generated among the most important cumulative findings in the study of ethnic politics. However, in a puzzling step backward, these findings are being conspicuously and comprehensively ignored in new research linking ethnic groups to political and economic outcomes. Contributors to the symposium discuss how the non-incorporation of constructivist findings affects the quality of our theories, and formulate proposals for how these findings might be incorporated into new research agendas.

### **Constructivist Findings: Ethnic Groups are Fluid and Endogenous**

Although “constructivism” and its opponent “primordialism” are now familiar labels, the meaning associated with each is rarely consistent. As a result, the distinctions between them are often obscured. In order to identify the key constructivist findings, therefore, it is necessary, at the risk of boring the reader, to first define the primordialist view against which they were formulated.

In general, the “primordialist” view is found more often as a commonsensical assumption informing arguments about other questions than as an explicit argument in itself. One of the few texts that explicitly lays out a primordialist view is Geertz’s 1973 essay “The Integrative Revolution” (Geertz 1973).<sup>1</sup> The two defining propositions of primordialism as they emerge from Geertz’s account are that 1) individuals have a single ethnic identity and 2) this identity is fixed in the present and future. The initial origins of this “fixed” identity are uncertain. For some primordialists, it is biologically determined. Others allow that it may initially have been constructed through human intervention. For Geertz, today’s “given” identity may have been “assumed” at some point in the past. For others, it may be the result of “ancient hatreds” born of conflicts centuries ago. The key distinguishing aspect of the primordialist view is that an individual’s ethnic identity becomes immutable once acquired, no matter where it comes from. The implication is that the ethnic groups to which individuals belong, and the ethnic demography made up by some collection of ethnic groups, can be unambiguously identified and taken as fixed in the long term. Further, we can safely take ethnic groups and ethnic demography as exogenous variables in our theories linking them to political, social and economic outcomes, since they exist prior to and independent of these outcomes.

The constructivist approach, developed across the disciplines of anthropology, sociology, political science, history and literature, has discredited the primordialist approach by showing that ethnic groups are fluid and endogenous to a set of social, economic and political processes. Those who subscribe to the constructivist approach agree on two basic propositions: First, individuals have multiple, not single, ethnic identities; and second, the identity with which they identify varies depending upon some specified causal variable. Changes in the value of these causal variables are likely to lead to changes in individual identifications. As individual identifications change, the ethnic groups and the ethnic demography that describe these individuals also change. The implication is that theories linking ethnic groups and ethnic demography to

outcomes first need to ask *which* of the many potential ethnic groups with which individuals might identify, and *which* of the many ethnic demographics that might be constituted by shifting group memberships, is relevant for the outcome they wish to explain. Further, no matter how they are identified, they need to ask whether or not the groups in question are somehow endogenous to the outcome of interest.

There are many variants of constructivism, however, and not all of them imply that *all* theories linking ethnic groups to outcomes are characterized by problems of shifting group identifications or of endogeneity. One variant identifies processes related to modernization as the key variables leading to ethnic group (or nation) formation (Gellner 1983, Anderson 1983, Deutsch 1953). This variant implies that currently salient ethnic groups can be taken as fixed and exogenous variables in explaining outcomes within and un-

related to a given level of modernization. The problems of shifting group identifications and endogeneity would arise only in explaining outcomes related to changes in the level of modernization. A second variant emphasizes institutions, especially those associated with the colonial state, as the key variable determining the salience of a particular ethnic identification (for instance, Laitin 1986, Fox 1985, Pandey 1992). This variant implies that currently salient ethnic groups might be taken as fixed and exogenous variables in explaining outcomes unrelated to and within a given institutional context. Questions of shifting group identification and endogeneity would arise principally in explaining outcomes re-

lated to changes in the institutional context. A third variant argues that particular ethnic identifications arise as the most efficient response to individual needs to extract desired patronage goods (jobs, markets and land) from the state (Bates 1974). This variant suggests that currently activated ethnic groups may be taken as obvious and exogenous variables in explaining outcomes within a specific type of state-dominated economy. However, questions of shifting group identification and endogeneity should arise in explaining outcomes related to changes in this economic context. A fourth variant highlights political entrepreneurship as the key variable in the construction and reconstruction of ethnic groups (Brass 1974, Kasfir 1979). This variant predicts a more pervasive problem of shifting group identification and endogeneity, even within fixed historical, institutional and economic contexts.

I have focused above on the “classic” constructivist texts, and do not cover several more recent variants, each of which raises the problem of group identification and endogeneity for some types of analyses but not others. Taken together, constructivist approaches demand that any analysis linking ethnic groups to outcomes *must at least raise the question of whether there are problems of shifting group identification and endogeneity in our theory-building enterprises*, even though we might well find that, for particular types of enterprises, the answer is that there are not.

#### **The Non-Incorporation of Constructivist Findings**

The constructivist findings that I describe above can be said

to have been more or less established across disciplines by the 1980s, although many of the authors associated with this approach had begun writing earlier (for instance, Barth 1969). It is now virtually impossible to find a social scientist who openly defends a primordialist position (Steve Van Evera’s essay later in this symposium is a notable exception). However, while everyone now pays lip service to constructivism, constructivist assumptions remain comprehensively unincorporated into our theories linking ethnic groups to outcomes.

Take for instance, our classic theories linking ethnic groups to democratic stability: Lijphart’s theory of consociationalism, and Horowitz and Rabushka and Shepsle’s models of ethnic “outbidding” (Lijphart 1977, Rabushka and Shepsle 1972, Horowitz 1985). These theories assume that the ethnic groups in question are fixed and not themselves subject to redefinition through the political process. Since these theories were developed before or at the same time that constructivist ideas emerged, their retention of primordialist assumptions is not surprising. However, it is necessary to ask whether their predictions would be affected in any way by subsequent constructivist advances. Later in this symposium, Lijphart attempts such a reassessment.

Remarkably, however, even new research on ethnic politics, conceptualized and executed in the wake of the constructivist advance, overlooks its implications.

Take for example one of the most fertile “growth areas” in research on ethnic politics: the application of IR approaches to ethnic violence, which is generating a

voluminous body of books, articles and dissertations (Posen 1993, Fearon 1998, Kaufmann 1996, Van Evera 1994). Posen explains war between ethnic groups in an environment of state collapse as a consequence of a “security dilemma” analogous to that which characterizes states in an environment of anarchy. Fearon, using the same analogy with “anarchy,” explains ethnic war as the result of a “commitment problem.” In Fearon’s argument, the majority ethnic group cannot credibly commit to protecting the rights of the minority in the future. Anticipating that its rights will be trampled on in the future, the minority group rebels in the present. Both arguments identify scenarios under which war between groups is more or less likely. But in all scenarios, both assume that the population is divided by one obvious line of cleavage; all individuals know which side of the cleavage they belong to and which individuals belong to the opposing side; all individuals agree upon this classification of themselves and others; and that this classification of self and other is exogenous and prior to inter-ethnic violence. Had these works taken the constructivist view seriously, they would have had to justify, rather than assume, that this particular configuration of ethnic groups was the relevant one for all actors in the conflict. Further, they would have had to establish that violence followed from these “pre-fabricated” group identities instead of itself being a variable affecting the formation of these identities.

Kaufmann’s argument identifying the separation of warring ethnic groups as a solution to ethnic war goes further than Posen’s and

Fearon’s in incorporating constructivist logic. Rather than asserting that ethnic groups are pre-fabricated entities that exist prior to ethnic violence, he makes a plausible argument that war hardens previously fluid identities and, once hardened, ethnic group identities can be hard to reconstruct under conditions of violence. However, his proposed solution does not follow the constructivist logic to its conclusion. The separation of warring groups into homogeneous ethnic enclaves, according to him, “eliminates both reasons and chances for ethnic cleansing of civilians (p. 137).” Steve Van Evera’s argument that peace is more likely when the boundaries of states and aspiring nations coincide (p. 11) supports Kaufmann’s plea for homogeneity as a means of reducing the risk of ethnic conflict. The plausibility of Kaufmann’s and Van Evera’s solutions depends upon the assumption that homogeneity, once created, is permanent. This view is inconsistent with a constructivist position. Constructivist advances suggest that ethnic homogeneity, like ethnic heterogeneity, is an artifact that can only be created and maintained under specific conditions. In order to take constructivist logic seriously, both Kaufmann and Van Evera would need to ask why homogeneity could be expected to persist once new states are created. How do we know that new bases for heterogeneity and therefore conflict would not arise as a consequence of socioeconomic changes, the creation of new political and economic institutions, and the emergence of new political strategies?

Consider a second growth area in research on ethnic politics: works, principally in the tradition

of political economy, which seek to identify the impact of ethnic heterogeneity on a range of political and economic outcomes. The literature on this question is voluminous, most of it published in the last decade. Examples include Ordeshook and Shvetsova (1994) and Cox (1997), who investigate the effect of ethnic heterogeneity on the number of parties; Easterly and Levine (1997) and Collier (1998), who investigate the impact of ethnic heterogeneity on economic growth in Africa; and Alesina, Baqir and Easterly (1999), who investigate the impact of ethnic heterogeneity on the distribution of public goods in American cities.

Ignoring constructivist advances, authors in this area all treat ethnic demography as an exogenous variable without probing for reverse causation. Ordeshook and Shvetsova’s remarkable assertion that ethnic heterogeneity, with some exceptions, “is not a product of individual choice – rather, it is better portrayed as an exogenously determined social state” (p. 108) is typical of this literature. In a partial exception to this rule, Alesina et al. make an effort to probe for endogeneity problems by testing to see if some third variable affects both public goods and ethnic divisions. However, the omitted variables they test for have no connection to any of the variables identified by constructivist theories as having a link to the process of group formation.

Second, the authors in this research tradition make no effort to assess the reliability of their data given constructivist insights. The examples cited above use three principal sources of data: A Soviet survey of ethnolinguistic groups published in 1964; the Mi-

norities at Risk Database (MAR) collected by Gurr et al for the period 1945-1989; and US Census data. In each case, the group categories and counts produced by these datasets are treated as self-evident and fixed for the entire time period of the analysis, which in some cases extends across several decades. In order to address constructivist advances, however, they would need to ask how these group categories and counts were arrived at and justify that this method in fact counted the relevant groups for the outcomes they wish to explain. Further, they would need to ascertain whether these categories and counts can in fact be taken as fixed for the entire period under analysis. Without asking these questions of the data, we have no means to assess their reliability for the question under study and design corrective measures where they are unreliable. However, the authors do not even raise these questions, let alone answer them. Later in this symposium, Laitin and Posner attempt such an interrogation of the Soviet data, showing how the method by which groups were counted should make us less confident about conclusions based on these data.

Consider, finally, recent work in political philosophy on "multiculturalism." One of the most widely cited of these works, routinely found now on syllabi on ethnic politics and democracy, is Will Kymlicka's *Multicultural Citizenship* (1995). Kymlicka's theory of minority rights equates "groups" with "cultures" and builds a justification for minority group rights on the premise that "cultures" deserve to be protected. This argument rests on the primordialist assumption that the "groups" in question are fixed,

constituted by individuals who have no countervailing group identities that might also become plausible candidates for recognition. Even if we accept the claim that "groups" are defined by distinct "cultures," this assumption is not sustainable given constructivist findings. In a constructivist world, in which individuals can potentially sort themselves into multiple groups, Kymlicka's theory would need to address the question of how the relevant "group" in need of protection should be identified; how to address the possibility that new groups and, supposedly, given the logic of his argument, new cultures, might emerge from old ones; and whether the institutionalization of group rights might in itself create new bases for inequality by privileging some mobilized group identities while making it harder for others to emerge. Here, as in the case of the IR and political economy approaches, the question is simply not raised. It is in this failure to even raise questions of shifting group identifications and endogeneity that the non-incorporation of constructivist advances is most blatant.

#### **Why and How Should we Incorporate Constructivist Findings in New Research Agendas?**

The essays that follow discuss the biases introduced in our theories by failing to incorporate constructivist findings, and propose methods of theory-building and data collection that take these findings into account. Arend Lijphart reassesses consociational theory in light of constructivist arguments. Incorporating constructivist assumptions into consociational theory, he finds, is complicated, but "has significantly enhanced the explanatory and pre-

scriptive value of consociational theory." Laitin and Posner evaluate the use of the Soviet data on ethnolinguistic groups in the political economy research programme to construct measures of ethnic diversity. They argue that constructivist findings "undermine not just the external validity of the Soviet data but the entire essentialist premise on which the data collection exercise was built," and present a proposal for designing measures of ethnic diversity which incorporate constructivist assumptions. Steven Wilkinson focuses on the implications of constructivism for our theories of ethnic violence. He argues that ignoring the constructivist critique will lead to "wrong conclusions about what causes ethnic violence, and about what measures might prevent it," and describes his own effort to collect data and test key propositions about ethnic violence using constructivist assumptions. Steve Van Evera sounds a note of caution. While accepting the argument that identities are constructed, he argues that the primordialist view is largely correct in assuming that they cannot be reconstructed. He proposes three hypotheses that might explain variance in the likelihood of reconstruction of identities across groups and situations. Finally, Ian Lustick's essay takes us beyond the problems that constructivism poses for previous analyses to identify the new avenues of research it opens up. He describes his use of computer-based modeling to identify questions raised by constructivism, which "have seldom if ever been posed, let alone answered," and introduces some early findings.

A final question remains: Why, if there is such a consensus

around constructivist findings, has new research in the past decade ignored constructivist findings so comprehensively? One answer, as several of the contributors to the symposium point out, lies in the fact that incorporating constructivist findings in data collection and theory-building efforts is extraordinarily difficult. But perhaps an even more important reason is that constructivist findings have not so far been codified. "Constructivism" in the study of ethnic politics is the product not of a single paradigmatic text but of a large, diffuse and interdisciplinary body of literature. While common findings may exist "out there" in this literature, they do not assume the status of "cumulative findings" until they are identified and inscribed as such. In the absence of such a codification, new entrants in this subfield are required each time to reinvent the wheel, and it is not surprising that many have ended up resorting to more primitive modes of transport. However, intellectual norms and procedures in the subfield of ethnic politics, and the subdiscipline of comparative politics more generally, do not encourage a system of periodic stock-taking, which might produce such a codification. Given the loss of knowledge that has resulted, in research on ethnic politics but no doubt also in other subfields, perhaps we should consider creating such norms and procedures.

### **Constructivism and Consociational Theory**

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How has the shift from primordialism to constructivism affected consociational (power-sharing) theory? During the first phase of my work on power-sharing, from the mid-1960s to the mid-1970s, the primordial view of ethnicity was still widely accepted in the social sciences, and I, too, accepted it without giving it much critical thought. In my *Democracy in Plural Societies* (Lijphart 1977), which was the culmination of this first phase, there are no less than five references to Clifford Geertz's (1963) *Old Societies and New States*.<sup>2</sup>

In my consociational writings from about 1980 on, my interpretation of ethnic identity has become mainly constructivist and, as a consequence, the explanatory power of consociationalism as well as its precision as a prescriptive model have been greatly improved. I was less influenced by constructivist scholarship, however, than by the constructivist lessons taught by a few problematic cases, especially South Africa and Lebanon. The Lebanese case raised the intellectual problem of how to explain the collapse of a power-sharing system that had worked reasonably well for more than thirty years. In South Africa, the intellectual and practical problem was to design an optimal power-sharing system in a country in which ethnicity and race were highly controversial questions.

These two cases led me to the formulation of the contrast between pre-determined and self-

determined groups in power-sharing systems. In my earlier writings, I had already emphasized that consociational democracy does not mean one specific set of rules and institutions. Instead, it means a general type of democracy in a plural (deeply divided) society defined in terms of four broad principles, all of which can be applied in a variety of ways. For instance, the grand coalition (sharing of executive power among representatives of all significant groups or segments) can take the form of a grand coalition cabinet in a parliamentary system or a coalitional arrangement of a president and other top officeholders in a presidential system of government, as in Lebanon. Segmental autonomy may take the form of territorial federalism or of autonomy of segments that are not defined in geographical terms. Proportional results in elections may be achieved by the various systems of formal proportional representation (PR) or by several non-PR methods, such as Lebanon's method of requiring ethnically balanced slates in multi-member district plurality elections. The minority veto can be either an absolute or a suspensive veto, and it may be applied either to all decision or to only certain specified kinds of decision, such as matters of culture and education.

Much more important than all these differences, however, is the contrast between pre-determination and self-determination of the constituent groups in a consociational democracy. Should these segments be identified in advance, and should power-sharing be implemented as a system in which these pre-determined segments share power? This appears to be the simplest way of instituting con-

sociationalism, but, as I discovered, it entails serious drawbacks. The alternative, which is necessarily somewhat more complicated, is to set up a system in which the segments are allowed, and even encouraged, to emerge spontaneously--and hence to define themselves instead of being pre-defined.

The Lebanese consociational system was established in 1943, but collapsed in 1975. A major part of the blame belongs not to internal problems caused by the power-sharing system itself but to Lebanon's precarious position in the international arena of the Middle East and, in particular, to repeated Palestinian, Syrian, and Israeli interventions. In this sense, the civil war that broke out in 1975 was not an ordinary civil war but an international conflict fought on Lebanese soil. However, the specific Lebanese form of power-sharing also had severe weaknesses because it was based on primordial assumptions concerning the fixed and stable nature of the sectarian segments (Maronite Christians, Sunni Muslims, Shiite Muslims, etc.). This left no room for secularly-oriented groups and individuals. Moreover, the relative shares of representation for the pre-determined segments were fixed on a permanent basis. The Christian sects continued to have greater representation and influence in the government in spite of the fact that the Muslims had gradually become the majority of the population, and the most powerful political office, the presidency, was permanently assigned to the Maronites.

The Lebanese themselves have recognized some of these problems and have tried to solve them. The 1989 Taif Accord changed the 6:5 ratio for parlia-

mentary elections favoring the Christians to equal parliamentary representation for Christians and Muslims, and it also roughly equalized the power of the Maronite president and the Sunni Muslim prime minister. But the pre-determined Christian sects are still over-represented, and there is still no provision for any other groups than the pre-determined sects.

Allowing groups to identify and define themselves is generally preferable to pre-determination for the following reasons: (1) Pre-determination is inevitably discriminatory--in favor of the groups that are included, and against groups, especially smaller groups, that are not recognized. (2) Pre-determination also entails the assignment of individuals to the specified groups, which may be controversial, offensive, or even completely unacceptable to many citizens. (3) It also means that there is no place for individuals or groups who reject the premise that society should be organized on an ethnic or communal basis. (4) Finally, in systems of pre-determination, there is a strong tendency to rigidly fix shares of representation on a permanent basis, such as the 6:5 Christian-Muslim ratio in pre-Taif Lebanon mentioned above. In contrast, self-determination is entirely non-discriminatory, neutral, flexible, and self-adjusting.

A good example of self-determination is provided by Dutch power-sharing established in 1917--the very first case of consociation that I analyzed (Lijphart 1968). The consociational engineers wisely based the new system on constructivist assumptions. Instead of using the four main population segments (Catholic, Calvinist, secular Socialist, and secular

Liberal) as the consociational building blocks, the system of educational autonomy was one of self-determination. All schools, public and private, were to receive equal financial support from the state in proportion to their enrollments. The new law was primarily designed to accommodate the main religious groups and their religious schools, but it was formulated in neutral language and allowed any group to establish and run schools as long as basic educational standards would be observed. As a result, it has also been taken advantage of by small secular groups interested in particular educational philosophies to establish, for instance, Montessori schools. And instead of assigning shares of political representation to each of the main segments on a pre-determined basis, a neutral PR system with a low threshold was adopted which allowed any group, not just the large "primordial" segments, to compete in elections.

In the South African case, the challenge for me and other consociationalists in the 1970s and 1980s was to propose an optimal consociational design for the unusual South African conditions. The main problem was that, while there was broad agreement that South Africa was a deeply divided society, the identification of the constituent segments was both objectively difficult and politically controversial. The old system of white minority rule had long relied on an official and strict classification of its citizens in four racial groups (African, White, Coloured, and Asian) and the further classification of the Africans into about a dozen ethnic groups. The racial classification served the allocation of basic rights; for instance, the short-lived 1983 "tricameral" con-

stitution excluded Africans from the national franchise and allowed the other three groups to elect separate chambers of parliament. The ethnic classification was the basis of the "grand apartheid" system of setting up, and encouraging the eventual independence of, a series of ethnic homelands. As a result of this policy of artificially forcing people into racial and ethnic categories, it had become quite unclear what the true dividing lines in the society were. A few observers also argued that industrialization and urbanization had had a melting-pot effect and that South Africa was no longer a plural society and had become a "common" society. Moreover, the White government's insistence on African ethnic differences had the ironic effect of weakening ethnicity because the homelands policy was widely despised. The African National Congress regarded ethnicity as a White divide-and-rule policy, and denied its existence and political relevance.

How could these disagreements about the identity of the segments and about whether South Africa was a plural society or not be resolved? The consociationalists' answer was that these disagreements did not need to be resolved, because a power-sharing system could be designed on the basis of self-determined groups. The key element was PR in a relatively pure form. The beauty of PR is not just that it yields proportional results and permits minority representation—two important advantages from a consociational perspective—but also that it permits the segments to define themselves. Hence the adoption of PR in South Africa would obviate the need for any prior settlement of divergent claims about its segmen-

tal composition. PR elections could also provide an answer to the question of whether and to what extent South Africa was a plural society or not, because PR treats all groups—ethnic or non-ethnic, racial or non-racial, and so on—in a completely equal and even-handed way. On the basis of the proportional election results, a proportionally constituted grand coalition government could then be prescribed by requiring that the cabinet be composed of all parties of a specified minimum size in parliament.

The Progressive Federal Party adopted these two proposals in 1978. The Buthelezi Commission, of which I was a member, also endorsed them in its final report issued in 1982. And they became the cornerstones of the first democratic and multi-racial South African constitution that went into effect in 1994. The minimum proportion of seats entitling parties to participation in the cabinet was set at a low 5 percent, and the PR system that was used in 1994 and again in 1999 was the purest and most proportional PR system for national elections used anywhere in the world, with an effective threshold giving a seat to a party with as little as one-fourth of one percent of the total vote. The cabinet formed in 1994 was a grand coalition of the African National Congress, the National Party (the ruling party in the old apartheid system), and Buthelezi's Inkatha Freedom Party. Mandatory power-sharing in the cabinet was abandoned in the permanent constitution that went into effect in 1999, but the cabinet continued to be a broad coalition of the African National Congress and the Inkatha Freedom Party. This system of self-determined groups has worked very well so far.

In South Africa, the legacy of apartheid made it impossible for a successful consociation to be built on any other basis than self-determination of groups. But the general proposition and recommendation that can be derived from the South African and Lebanese cases is that, because ethnic identities are very often unclear, fluid, and flexible, self-determination can always be expected to work better than predetermination. This constructivist-based proposition has significantly enhanced the explanatory and prescriptive value of consociational theory.

#### **The Implications of Constructivism for Constructing Ethnic Fractionalization Indices**

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In recent years, ethnic fractionalization has emerged as a central variable in quantitative analyses of outcomes ranging from economic growth rates (Easterly and Levine 1997) and the quality of governance (La Porta et al 1999) to the frequency of coups d'état (Londregan and Poole 1990). Almost all such analyses employ, either alone or in combination with other measures, the same measure of ethnic fractionalization. This index, called ELF (for Ethno-Linguistic Fractionalization), is available for 129 countries – indeed, its broad coverage is the principal reason for its widespread adoption – and reflects the likelihood that two people chosen at random will be from different ethnic groups. It is calculated using

the Herfindahl concentration formula from data compiled in a global survey of ethnic groups published in the *Atlas Narodov Mira* (1964) and subsequently included in Taylor and Hudson (1972).

Users of the ELF index have analyzed their results, to their peril, without any regard to the constructivist findings in the literature on ethnicity. Constructivist findings would make the standard ELF index suspect for four different reasons. First, the users of the ELF index assume that a country's degree of ethnic fractionalization is fixed, analogous to its topography or its distance from the equator. To the extent that a country's boundaries do not change, it is assumed, its ELF score should remain constant. Constructivist theories of ethnicity, however, would compel us to challenge this assumption. They would lead us to expect changes in the level of ethnic fractionalization over time, as people over generations assimilate, differentiate, amalgamate, break-apart, immigrate and emigrate.

Take the case of Somalia. At independence, Isaaqs (from former British Somaliland) and Hawiyes (from former Italian Somalia) insisted they spoke the same language, and any survey of linguistic diversity undertaken at the time would have reflected this. In recent years, however, Isaaqs have begun consciously differentiating their speech forms from those of the Hawiyes as part of an attempt to justify recognition for their secessionist republic—much as Croat and Serb intellectuals and linguists have done over the past fifteen years in the Balkans (Greenberg 2000). A linguistic survey conducted today would thus produce a quite different accounting of linguistic divisions in both for-

mer Yugoslavia and former Somalia. Clan distinctions in Somalia have undergone a similar metamorphosis. With the decline of the dictatorship of Mohammed Siyaad Barre in the late 1980s, what had previously been considered one of the most ethnically homogeneous countries in Africa became severely divided by inter-clan fractionalization, with a concomitant change in the level of aggregation that is considered appropriate by political analysts. Studies of Somalia in the 1960s that focused on clan-based divisions tended to concentrate their analysis at the highest level of division (the clan family), of which there are three. But amid the fractionalization caused by the civil war that broke the country apart a decade ago, more recent analyses have tended to emphasize distinctions among clans and even sub-clans. Thus, due to the civil war, a survey of ethnic fractionalization today would yield a substantially larger number of clans (and a correspondingly higher fractionalization index value) than one undertaken forty years ago. Contrary to the assumptions of most users of the ELF index, levels of ethnic fractionalization in Somalia have been dynamic over time, not stable givens of the landscape. Constructivist findings would thus seem to demand that fractionalization scores be provided over a time series to accommodate such changes.

A second reason that constructivists should be suspicious of the ELF measure as used today is that a single measure of ELF for a country misses the social reality that there are multiple dimensions of ethnic identity in all countries, and that polities have different levels of fractionalization on different dimensions. India's population,

for example, can be divided along religious, linguistic, caste or even state lines. Which of these four bases of group division should be used to count the groups that we plug into our fractionalization formula? Our decision matters tremendously for the value we arrive at: defined in terms of religious differences, India's ethnic fractionalization index would be 0.31; defined in terms of language distinctions, it would be 0.79.<sup>3</sup> Calculations based on class and state cleavages would yield different values still. Constructivist findings would seem to require a list of all groups and a separate ELF calculation for each dimension of ethnic difference.

Even if we were to collect the data required to calculate multiple indices for each country and time period, this would still leave us with the question of which measure to use, since there is no way to know *ex ante* which line of ethnic cleavage is likely to be politically important, and thus no way to privilege one cut on the data—and one ELF value—over another. Indeed, constructivist approaches going back to the work of Lipset and Rokkan (1967) point out that although the political salience of ethnic cleavages becomes institutionalized in party systems, this salience is an historical but not a natural phenomenon. Thus quite apart from the fact that the roster of groups on each cleavage dimension can change over time, we also need to find a way to accommodate the fact that the particular dimension of ethnic cleavage that matters for the outcome we are interested in explaining varies too. This suggests a third reason for constructivists to doubt the validity of a single ELF measure.

To illustrate this point, sup-

pose we are interested in investigating whether ethnic fractionalization is related to voting behavior in European democracies. How would we code France? In the Third Republic, religious cleavages were quite salient, and this would suggest that we would need to count up the shares of Catholics, Protestants, seculars, Jews, Muslims, and other religious groups so we could plug these values into our concentration formula. Today, however, racial cleavages are taking on a new significance, and so presumably we would need to build our fractionalization index from a very different set of groups and population shares.

A focus on salience raises a related issue. Once we have established which dimension of ethnic cleavage is salient, we still need to decide which groups we should include in our count. The reason this is an important issue is because while ethnic groups may exist "as such" in anthropological categorization, they may not have constructed themselves "for such" as political actors. On every cleavage dimension, we are likely to find dozens of groups that are culturally distinct from their neighbors but that are irrelevant as political actors in their own right. In some cases, this is because these groups fold themselves into broader political coalitions when it comes to competing over resources and national-level policy outcomes: Tongas, Lenjes and Toka-Leyas in Zambia become "Southerners;" Puerto Ricans, Cubans and Dominicans in Miami become "Latinos." In other instances, it is because they simply do not participate in politics as distinct, recognizable groups. Whichever the reason, including such groups in the data from

which the fractionalization index is calculated is problematic.

For example, suppose that, like Easterly and Levine (1997), we are interested in testing the effects of ethnic heterogeneity on economic growth rates. How would we code Kenya? Easterly and Levine's solution is to take the ELF value for Kenya off the shelf. This entails using a fractionalization index that was calculated from a count of twenty-one different ethnic groups. Yet, by their own account, the distorted macroeconomic policies that explain Kenya's low growth rate are generated by the competition between just three broad ethnic coalitions: the Kalenjin, the Luo and the Kikuyu, each of which is described as containing "a third of Kenya's population." If, as Easterly and Levine claim, it is the competition among these three groups that is affecting Kenya's rate of economic growth, then the appropriate fractionalization index should be calculated from the population shares of these three groups rather than from the relative sizes of the twenty-one. The point is that to capture the contribution that a country's ethnic heterogeneity makes to such a process requires an index of fractionalization that reflects the groups *that are actually doing the competing*. One of the most important problems with the ELF index is that, more often than not, it does not do this: as in the Kenya example, it includes dozens of groups that are irrelevant to the process that it is employed to capture.

A final issue raised by the constructivist literature is the possibility of endogeneity. The ELF index is prized in econometric analyses in part because it is assumed to be exogenous to the outcomes it is used to explain. Yet as

constructivist findings suggest, this is not always the case. To revisit our Somalia example, if the collapse of the dictatorship is what compelled Somalis to redefine their group boundaries, then using the *ex post* ethnic landscape that those boundaries now define to explain the earlier breakdown of the Somali state - as scholars might reasonably be tempted to do - would be a methodological error. Or take the example of the U.S., whose linguistic homogeneity can be explained in large part by the economic benefits of speaking English and the sense of security immigrants have that they will not be sent back unwillingly to their homelands. High rates of linguistic assimilation in the U.S. are a result of political stability and economic prosperity. Arguing that linguistic homogeneity explains the U.S.'s economic performance or stability would have the causal arrows going in the wrong direction.

In sum, constructivist theory demands that the ELF measure, ubiquitous in econometric accounts of economic growth, ethnic violence, political stability and other outcomes, should be disaggregated by time, by cleavage, and by salience, and that those who employ it consider the possibility of endogeneity. Economists would be incredulous if a scholar plugged in a single economic variable, say the rate of inflation in 1945, and thought that it was a good measure of a country's level of prosperity in 1990. They would be similarly incredulous if the scholar then used the measure to explain outcomes like democracy or political stability, which are often postulated as causes of economic prosperity. Constructivists should be equally nonplussed when a "one size fits all" measure of

ethnic fractionalization, taken at a single point in time, on a single dimension, and with no attention to its salience or its potential endogeneity, is used as an explanation for consequential political outcomes.

### What Can Be Done?

The implications of this constructivist critique of the standard ELF index for data collection are immense. To start, we would need to construct a list, for each polity, of all of the ethnic cleavages understood by members of the population to be meaningful axes of social differentiation. Such a list would vary from country to country but would probably include language, tribe, clan structure, caste, race and religion. We would then need to identify, for each line of ethnic cleavage, both the categories into which people are divided and the percentage of people within each category. Thus if the dimension of ethnic cleavage is "world religions," we would need to know the percentage of Christians, Muslims, Hindus, Buddhists and Jews. Note, however, that many of these categories are themselves sub-dividable: within the "Christianity" category, a number of additional distinctions might be relevant - for example, among Eastern Orthodox, Roman Catholic, and a range of Protestant affiliations. Therefore religion might involve more than one dimension in a country. We would also need to know which of these categories is politically salient for different kinds of issues and different loci of competition. Indeed, we should emphasize that the politically salient dimension may be different within a sub-unit of a polity from the polity itself. For example, it may be the case that race is a consequential dimen-

sion in U.S. politics, but is not salient in Minnesota. An ethnic fractionalization score for Minnesota (in a study of the various U.S. states) might be computed on the basis of religious denomination while a cross-national study might compute fractionalization in the U.S. to be based on national origin or race. Finally, we would want periodic re-scoring of these fractionalization scores to build up a longitudinal dimension.

Many constructivists, while sympathetic to our call for greater appreciation of ethnic complexity, will bristle at the idea that ethnic identities could actually be measured in the way that the data collection program that we propose would require. They would argue that we have learned that ethnic identities are situational, driven by context, and that it is therefore impossible to divide a population into categories of identity in any time period. A saleswoman in a Kenyan market might present herself as a Luo to a customer speaking that language (as her mother was a Luo-speaker), as a Kikuyu to a customer in an expensive suit (as her father was a Kikuyu), and as a Swahili to her neighbor in the market (as Kiswahili is the lingua franca of East African tradespeople). An American social scientist who asked for her ethnic identity, might get "Kenyan" as a response. Our trader, when asked for her ethnic identity, might in different contexts answer with Luo, Kikuyu, Swahili, or Kenyan. Constructivists will point out that all of these answers are correct, at one and the same time, and that such complexity undermines any attempt to categorize a population ethnically.

We do not think these observations are damaging to our proposed data gathering exercise.

There are usually clear rules for self-definition ethnically. If a patrilineal descent rule is practiced in Kenya, this market woman would be categorized as Kikuyu, despite her clever move to win a sale to a Luo customer by portraying herself as one of his kin. She may speak Swahili, but if pressed would hardly consider herself Swahili by ethnicity. This could be confirmed by further observation, for example watching as she is excluded from ethnic Swahili trade networks. And so, for tribal identity, we could code her correctly as Kikuyu, though we might also code her linguistically as a Swahili speaker.

As for her last answer (Kenyan), it is important to know the extent to which new national identities are forming, and becoming ethnicized. We would guess that a Kenyan ethnic identity would be evoked in some contexts, for example answering questions to a foreigner, or complaining about resources being spent on Somali refugees that are spent at the expense of "genuine Kenyans." To the extent that a Kenyan identity gets evoked in many contexts, we would begin to see, at least on one important measure, a reduction in the level of ethnic heterogeneity in Kenya, as occurred for example with the creation of Frenchmen in Third Republic France. If "Kenyan" vs. "foreigner" is a cleavage in Kenyan society, we should make sure this is one of the dimensions of ethnic division on which we collect data. Ethnicity may be situational, but there are rules in each society how best to code people, and these rules should become a basis for coding percentages in a revised set of fractionalization indices.

Theories that posit some eternal presence of ethnic groups will

be satisfied with the ELF measures derived from the work of the Soviet geographers who assiduously counted the world's ethnic groups for each country in the 1950s and 1960s. Constructivist theory teaches us that the assumption of eternal membership is flawed. If we want to build better models of the relationship between ethnic diversity and economic growth or political stability or the quality of governance, it is essential to commit ourselves, as a discipline, to the collection of data that validly represents the multiple dimensions of ethnic diversity found in each country, and does so over time. This is an immense challenge to our field. But it is more than justified by the cumulative findings of the last thirty years of work on ethnicity. This research undermines not just the external validity of the Soviet data, but the entire essentialist premise on which the data collection exercise was built. Econometric analyses that aim to test the effects of ethnic diversity will need to take regard of constructivist findings and seek better-conceived data on ethnic fractionalization than they are currently employing.

### Constructivist Assumptions and Ethnic Violence

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It is easy to get depressed about the implications of constructivist insights for the study of ethnic violence. If we accept some of the key constructivist ideas—that individuals have multiple ethnic identities, whose salience changes over time and in different con-

texts—then where does this leave the many theories of violence that assume that ethnic groups are easily measured and stable? Should we simply throw out those explanations that use a country's or a town's ethnic balance—whether measured through percentages or ethnic fractionalization indices—as a key explanatory variable? And what are we to do with “security dilemma” models of ethnic violence, if they are premised on the existence of solid and threatening ethnic groups in which we no longer believe?

One response—comforting to those of us who have invested a great deal of time and effort in data collection—is to claim that the constructivist critique is not so serious that we need to revise our basic measures of ethnic identities and ethnic violence, or the theories we have developed using these measures. Even if we grant that the constructivists are correct in pointing out that *some people* have multiple identities whose salience changes in response to economic incentives, violence, and institutional constraints, the overall proportion of such multidimensional “switchers” may be so small in practice that our basic census-derived measures of ethnic identity might still be reliable.

However, the proportion of those who switch among multiple identities is probably not small. The evidence suggests that even after instances of large-scale violence or state-sponsored polarization, a large number of people continue to have what Mary Waters terms “ethnic options” (Waters 1990). In the mixed German/Czech town of Budějovice, for example, one researcher has found that even after riots in the late 19<sup>th</sup> and early 20<sup>th</sup> centuries,

11% of the town's 40,000 “Czechs” could still switch and redefine themselves as “German” in the early 1940s. If we ignore both the multiplicity of ethnic identities that individuals can invoke and such periodic shifts, we will inevitably come to the wrong conclusions about what causes ethnic violence, and about what measures might prevent it.

Such changes in the salience of ethnic identities, furthermore, are seldom accidental. Provoking violence, and then ensuring it is labeled appropriately, is often the *means* by which political entrepreneurs try to mobilize constituents around one (politically advantageous) ethnic identity rather than another (Brass 1997). In 19<sup>th</sup> century Ireland, for example, Episcopalian Tory politicians provided financial and organizational support for Orange marches through Catholic neighborhoods in order to provoke a defensive reaction from Catholics that would help rally Methodist and Presbyterian voters to the Tory party, and therefore preserve Episcopalian dominance in politics. So to see these “Protestant-Catholic” riots purely as evidence of the strength of the 19<sup>th</sup> century Protestant-Catholic religious cleavage, and to explain them using such measures as the inter-religious population balance reported in the subsequent census, would be to confuse the theoretical cause of ethnic violence with its effect.

Consider the problems in Easterly's recent statistical study of ethnic violence since the 1960s, which concludes that “ethnic fragmentation has a significant and positive effect on the probability of genocide, while the interaction term between ethnic fragmentation and institutions has a negative

effect.” (Easterly 2000) Potentially these findings might be used to develop models showing why more diverse societies have more violence as well as to recommend a whole host of institutional reforms aimed at encouraging economic growth and reducing ethnic violence. The problem, of course, is that the core findings about the relationship between ethnic diversity and violence are most likely unreliable. The study relies upon the widely used Ethnolinguistic Fractionalization Index, which, as Laitin and Posner point out in this symposium, covers some dimensions of ethnic identity but not others, and is based on data from only a few years. There is also the endogeneity problem: the study explains violence over several decades using data on ethnic fractionalization compiled from only a few years’ worth of census statistics in the 1960s, even though we know that the salience of ethnic identities probably changed over time in response to both violence and institutional incentives.

Ignoring the fluidity of ethnic identities has very definite implications not just for assessing our measures of ethnic diversity, but also for assessing the value of public policy proposals made to reduce ethnic violence. Once we accept that ethnic identities are multi-dimensional, we ought to develop a healthy skepticism about power sharing proposals premised—like the 1998 Good Friday agreement in Northern Ireland—upon the existence of solid ethnic groups. Does it really make sense to grant a linguistic or caste group’s leaders a particular share of jobs or political power, and a minority veto over future constitutional reforms when it is likely that identities will change over time

and that cleavages within a group will emerge that will lead to new conflicts over the distribution of scarce goods? In India, for instance, the government has tried to moderate conflict through creating linguistic states for different groups and special benefits for middle and lower castes. But because the division of scarce resources within each of these “homogenous” units is often highly unequal, and because this inequality often follows sub-ethnic lines, new ethnic conflicts have continued to emerge. In south India, riots have recently broken out between sub groups of the Scheduled Castes (untouchables) over the division of government positions, spending and places in higher education. When these benefits for the ex-untouchables were first introduced in the 1950s the poorer groups within the Scheduled Castes voiced no opposition to being counted as part of the broad lower caste category. But in 1998, violent conflicts broke out in southern India as some sub castes angrily demanded that they be given separate job and spending quotas on the grounds that most of the Scheduled Caste jobs and university places were being filled by members of only a few well placed sub castes. The central challenge posed by constructivism for ethnic conflict moderation is how to design proposals that address the concerns of existing groups while providing simultaneously for the possibility that the groups themselves might be redefined over time.

#### **Incorporating Constructivism into the Study of Ethnic Violence**

After all these critiques, can we say anything constructive about constructivism? Can ideas about multi-

ple shifting identities be used to help us develop better measures of key concepts and better answers to our most important questions? Or is constructivism only useful for knocking down the theories and operationalizations of others?

It is possible, I think, to incorporate constructivist assumptions into our own theory testing and data gathering about ethnic violence. We need to collect new data that measure multiple identities (both ethnic and non-ethnic) over time. These data should include variables on such factors as the creation of federal units or government spending programs for minorities, which theoretically we believe will encourage the salience of some identities rather than others. Our data should also include information on the shifting and multiple identities associated with ethnic violence, so we can begin to determine whether political mobilization around ethnic identities really poses, as many have argued, a qualitatively different threat to the state than ideological or socio-economic movements.

Only with these new data on the salience of multiple identities over time can we begin to investigate some of the most interesting questions in the study of ethnic violence: Does it really matter if one particular ethnic or non-ethnic identity is salient at a particular time? Is ethnic mobilization inherently more dangerous—as we often assume—than mobilization around class ideologies or socio-economic interests? (Elster et al 1998, 249-51) Are some forms of ethnic mobilization—for example around language—more or less likely to lead to violence than those around say, religious or tribal identities (Laitin, 1999)? And to what degree is it really true

that the occurrence of ethnic violence and ethnic civil wars “freezes” ethnic identities for years afterwards (Kaufmann, 1996)?

I have tried to make a start in developing data to address these questions by collecting statistics on reported group mobilization and violence in India, based on a random sample of newspaper reports from several hundred days from each decade since 1950. So far I have finished collecting data from the 1950s, 1960s and 1990s. For each day in the sample I record every reported instance of every type of group mobilization, including strikes, processions, party rallies, violent demonstrations and organized physical attacks. Then I record *all* the reported motivations for each demonstration or act of violence. If a procession in Bombay, for example, was organized by the Communist Party around women’s issues, I list it as being motivated by two identities, “gender” and “political party.” If there is a demonstration by the Hindu nationalist Bharatiya Janata Party against cow slaughter I report the motivations “religion” and “political party.”<sup>4</sup>

This method does have some limitations. For example, it does not permit us to tell whether a particular ethnic or non-ethnic identity was the primary cause of a demonstration, strike or riot. But if—as is the case—my aim is to find out about broader patterns of mobilization and conflict *over time*, and about differences between different types of ethnic mobilization, then the method offers clear advantages. Because we exclude none of the information about mobilizing identities, we should be able to get a much clearer picture of how aggregate patterns of political mobilization (both ethnic and

non-ethnic) have changed over time, about which ethnic identities are salient in different decades (as a result of violence or institutional incentives), and about which ethnic and non-ethnic identities are most likely to be associated with high violence.

From the several decades’ worth of data collected so far there are four preliminary findings. First, ethnic mobilization has become more common in India. In the 1950s 44% of events were reported as being motivated by ethnic identities, compared to 55% in the 1990s. Second, within the broad category of “ethnic” mobilization there has been a substantial shift since the 1950s in the particular ethnic identities around which Indians mobilize. “Language,” which accounted for 25% of all ethnic mobilization in the 1950s, has dropped to only 2% in the 1990s. Mobilization over religious and caste identities, meanwhile, has sharply increased. Third, ethnic mobilization in India does seem to be associated with a higher level of deaths and injuries than other types of political mobilization. Fourth some kinds of ethnic mobilization do seem to be associated with higher levels of violence than others. The variable “Religion” (which codes for all instances of religious mobilization), for example, is positively associated with the level of both injuries and deaths, whereas caste, language and tribal mobilization are negatively related or unrelated (depending on decade) to deaths and injuries.

How do we explain the association between religious mobilization and violence, and the relatively benign character of caste, language and tribal mobilization? The fact that India’s neighbor

Pakistan has had several major *linguistic* conflicts since 1947 should make us wary, I think, of too quickly interpreting these results as proof that religious mobilization is inherently more of a threat than movements around language. A more plausible argument, I find, is a historical-institutionalist argument that focuses attention on the policies pursued by the post-colonial Indian state. The British colonial government’s institutionalization of religious identities and the consequent religiously based partition of India in 1947 gave India’s post-independence leaders a profound suspicion of any claims made against the state on the basis of religion. Consequently, the post-1947 Indian state has treated religion-based mobilization as illegitimate while it has been willing to accommodate claims made on the basis of language and other identities. In practice, this means that the Indian government has met quite moderate demands by religious movements—the Sikhs in the state of Punjab and the Muslims in the state of Jammu and Kashmir—in a much more repressive way than it has approached the demands of the Tamil and Telegu speakers in southern India. Through their intransigence, successive Indian governments have therefore driven many Sikhs and Kashmiris to join armed militant organizations. The attacks launched by these organizations, and the counterattacks launched by the government, have been responsible for most of the upsurge in deadly ethnic violence in India in the 1990s. The argument suggests that the roots of ethnic violence lie, not in the intrinsic character of ethnic groups, defined on the basis of religion or otherwise,

but in the institutional context in which these groups interact with each other and the state.

### Primordialism Lives!

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Should we take ethnic groups as fixed for the purpose of political analysis? In other words, is it sensible to assume that ethnic identities cannot be reconstructed?

The constructivist claim that ethnic identities are socially constructed is clearly correct. After all, our ethnic identities are not stamped on our genes, so they must be socially constructed. It does not follow, however, that we should drop the assumption of fixed ethnic identity. This is because ethnic identities, while constructed, are hard to reconstruct once they form (Connor 1994, Smith 1986). Reconstruction can happen but the conditions needed for reconstruction are quite rare, especially in modern times, and especially among ethnic groups in conflict. Hence the reconstruction of ethnic identities is seldom possible, and the reconstruction of identity can seldom serve as a remedy for ethnic conflict today. It therefore makes sense to retain the assumption of fixed ethnic identity for most analysis, especially for analysis of the causes and prevention of ethnic conflict.

In other words, our editor Kanchan Chandra has successfully smoked a (horrors!) primordialist out of hiding. My primordialism is qualified: I think primordialist ideas do not fit all ethnic identities or situations. But I do argue that primordialism—as Chandra sum-

marizes it—covers most modern identities and most identities in conflict situations. In short, the primordialist view has been prematurely dismissed and deserves a second look.

Three claims elaborating this argument, and a fourth point of qualification, are advanced below. (1) Ethnic identities harden when mass literacy is achieved. (2) Ethnic identities are hardened by violent conflict with others. (3) The identities of non-immigrant ethnic groups are far more firmly fixed than immigrant identities. (4) While ethnic identities can seldom be transformed into new identities, they can often be made more benign, and efforts in this direction can bolster peace.

1. Ethnic identities harden when mass literacy is achieved. The identities of non-literate people can be reshaped, but the identities of most mass-literate peoples are quite firmly fixed (Byman 2000, 154). This is because mass literacy allows the identity to be stored in writing and purveyed in common form to a mass audience. Storage in writing and purveyance in common form give an identity a more developed and uniform character than an identity stored in an oral tradition. Both development and uniformity in turn bolster the identity. A more developed identity—one that includes more historical and cultural content—inspires more loyalty because it offers more substance to be loyal to. A group with a more uniform identity is less weakened by local variation in the identity: group members find themselves sharing a more closely identical culture even with group members who are strangers, which bolsters group solidarity.

Written identities also have a

resilient quality that makes them almost impossible to stamp out. The identity may lie dormant for a time, but ethnic entrepreneurs can bring it back by recovering and purveying the nation's holy texts. This is why some modern ethnic identities have been Phoenix-like, subsiding for decades and then roaring back to life. Scottish, Welsh and Breton nationalism all illustrate.

The hardening effect of mass literacy on ethnic identities is seen in the dramatic slowing of ethnic assimilation in regions where mass literacy has arrived. Eurasia's past is littered with vanished pre-literate identities. The Hittites, Sumerians, Babylonians, Phoenicians, Amorites, Edomites, Moabites, Jebusites, Ostrogoths, Visigoths, Parthians, Picts, Gepids, Getes, Vandals, and many more have disappeared into the mists of history. But once literacy began spreading in the nineteenth century the vanishing of Eurasian identities became much less common, and it almost ended among groups with broad literacy and a written history and culture. One could even argue that this class is an empty set—that there are no clear examples of major mass-literate Eurasian identities that have vanished.

Nineteenth-century France made peasants with local identities into Frenchmen before these local identities were crystallized and broadly propagated through writing (Weber 1974). Nineteenth-century Italy did the same. Today such identity reconstruction would be far harder in Europe because Europe's cultures have been vastly hardened by being recorded and widely published. The story shows that identities are often etched into stone when they are printed on paper.

2. Ethnic identities are hard-

ened by violent conflict with others. Conflict enhances the hardening effect of mass literacy on identity by enhancing the emotional impact of recorded national memories (Kaufmann 1996, Byman 2000). The experience of warring or oppressed peoples, filled as it is with tales of common struggle and sacrifice for the common good, creates a stronger we-feeling than the experience of people who escape these tragedies; hence it has stronger effects when national scribes record and purvey it.

For this reason groups in conflict are especially poor candidates for identity change, and identity change is an especially unlikely remedy for ethnic conflict.

3. The identities of non-immigrant ethnic groups are far more firmly fixed than the identities of immigrant ethnic groups. Immigrant groups often assimilate, remaking their identities in the process. Thus the ethnic identities of many immigrant groups in major immigrant states—the United States, Argentina, Chile, Brazil, and English-speaking Canada—have largely faded. But non-immigrant groups assimilate far less often, and in modern times ethnic identities in Africa, Asia, and Europe—which are peopled largely by groups that do not see themselves as immigrants—have been much more enduring. The identities of peoples living in immigrant-majority states but were not voluntary immigrants themselves—such as the Quebecois and native Indians of Canada, the African-Americans and native Indians of the United States, and many native Indians of Latin America—have also proved durable.

Three explanations for this empirical regularity suggest themselves. A moral explanation sug-

gests that people accept the duty to compromise their culture when by immigrating they willingly choose to live with people of other cultures.<sup>5</sup> An immigrant self-selection explanation (Byman 2000) suggests that individuals with weaker ethnic identities are more willing to leave their homelands to emigrate; and that cultural leaders are especially unlikely to emigrate because they abandon their personal sense of identity by doing so. As a result immigrant cultures are comprised of weak ethnic identifiers, and they lack cultural leaders; hence they assimilate easily. Finally, a state-selection explanation (Byman 2000) suggests that states with liberal and tolerant cultures become immigrant states because they allow more immigrants to enter than more xenophobic states, and because immigrants prefer to move to these more tolerant states. Then, goes the argument, these tolerant states proceed to treat their immigrants better; hence the identities of their immigrants, unhardened by oppression, fade more easily. In this view immigrant states are different to start with, having aspects that are conducive to identity change.

Whatever the explanation for this empirical regularity, its implication is that the assimilation of immigrant groups in the United States is atypical of the wider world, so extrapolation from the U.S. experience is misleading. Most important, it suggests that prescriptions drawn from the U.S. experience will likely fail in non-immigrant societies. Americans cannot bring ethnic peace to other lands just by advertising the American example of mutual ethnic tolerance. Ethnic groups in Bosnia, Iraq, Sri Lanka and the

Caucasus cohabit in a non-immigrant setting to which American experience is largely irrelevant. Simply hectoring them to adopt American habits of non-discrimination will not improve their relations.

In summary to this point: mass literacy, violent conflict, and non-immigrant character are all barriers to identity change. I further think these factors interact in synergistic fashion. Specifically, groups with any one of these attributes may still change identities, but groups that are both mass literate and are either non-immigrant or are in violent conflict with others almost never change identity. Since most of the world is now both mass-literate and non-immigrant, and will increasingly fit this description as literacy spreads further in the years ahead, we should expect little identity change in the future. And we should expect little identity change among mass-literate ethnic groups in conflict. Hence we must seek to remedy most ethnic violence by means other than identity change.

4. Ethnic identities can be made more benign. The basic direction of most identities—an identification as German or Basque or Croat or French—is quite fixed, but the texture or flavor of identities can be reconstructed. Specifically, identities can be remade from malignant to benign. For example, German nationalism has been dramatically remade from hegemonic to tolerant since 1945. Germany once viewed its neighbors as dangerous and inferior and claimed a right to dominate them. That worldview has been replaced by a tolerant German nationalism that claims no right to dominate. Other Euro-

pean nationalisms have also assumed a much more benign aspect since World War II, mainly because Europeans have largely abandoned the spreading of chauvinism through the schools and moved to commonize their teaching of European history.

Non-immigrant mass-literate groups are bound to remain who they are. Serbs will be Serbs and Croats will be Croats. Neither will assimilate to being something else. But Serb and Croat nationalism can be tempered into something more benign. The German example shows how far this tempering can go and how much can be achieved by nurturing it. Redirecting identities is usually a Quixotic project but re-flavoring identities shows great promise as a palliative to ethnic conflict.

### **What about multiple identities and permeable identity boundaries?**

In addition to claiming that identities are constructed, constructivists make two further claims in support of arguments that identities are not fixed: that individuals have multiple identities, and that ethnic identities have permeable boundaries. Both claims are weak.

Individuals do have multiple identities, but in modern times ethnic identities tend to become paramount; and once they become paramount, ethnic identities tend to remain paramount. The worldwide trend of the past two centuries has been toward giving greater loyalty to one's national or ethnic group, while giving less loyalty to one's clan, region, religion, state, or—since the collapse of Marxism—one's political ideology. Exceptions can be found to this rule but it is a strong tendency.

There is seepage around the

edges of most ethnic groups. Even groups with firm identities see some intermarriage with other groups. But this gives us little hope for managing ethnic conflict. Intermarriage among Serbs, Croats and Bosniaks was fairly common in Bosnia before 1992 but this did little to slow the Bosnian slaughter of 1992-95. The lesson is that ethnic groups and ethnic conflicts can survive a good deal of intermarriage. If so, the permeability of ethnic boundaries that constructivists observe must be very large to offer a solution to ethnic conflict.

Those who underestimate the strength and endurance of ethnic identities are bound to blunder in their dealings with nationalism. Hence the constructivist tendency toward this underestimate is dangerous as well as incorrect.

For example, United States foreign policy has often erred because it underestimated the strength and endurance of ethnic identities. The U.S. launched its Cold War intervention in Vietnam (1961-73) partly because Americans failed to realize that they would collide with a powerful Vietnamese nationalism too strong to overcome. Other U.S. Cold War interventions—in Iran, Guatemala, Chile, the Dominican Republic, Angola, Nicaragua, Cambodia, El Salvador, and elsewhere—were launched to prevent the Soviet Union from extending its empire, in ignorance that powerful Third World nationalisms already made a wider Soviet empire impossible (as the Afghan resistance showed during 1980-1989). Later the U.S. bungled in its efforts to prevent and then manage conflicts in the Balkans because it failed to appreciate the power of the ethnic identities in

that region. U.S. troops are still policing the 1995 Bosnia settlement because that settlement was naively premised on the expectation that Bosnia's three ethnic groups would curb their identities and learn to get along.

Constructivist arguments that downplay the strength and endurance of ethnic identities thus move U.S. thinking in the wrong direction. Americans have erred far more often in underestimating than overestimating the strength of these identities, with tragic results.

### **Agent-Based Modeling and Constructivist Identity Theory**

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One blessing of a research program is the instruction it gives us about the questions we need not, indeed cannot, ask. In the study of political identity the constructivist research program has liberated scholars operating within its heuristic boundaries from having to consider or refute “primordialist” or “essentialist” positions. And so I shall not. This is a major accomplishment of constructivism, writ large to include approaches or “schools” often described with terms such as “instrumentalism,” “circumstantialism,” “hegemonism,” “perennialism,” or “strategic manipulation.” But all these are simply variations on the fundamental constructivist assumptions. Each tends to emphasize a different aspect of constructivism or a slightly different assessment of the relative importance of such crucial corollaries of constructivist theory as identity multiplicity, fluidity, incentive responsiveness, strategic manipula-

bility, or entrepreneurial efficacy.

Nevertheless, with few exceptions, the vast literatures in anthropology, political science, international relations, cultural studies, sociology, and literary criticism that apply constructivist principles have done so with a fairly standard set of objectives in mind. Overwhelmingly, and now even deadeningly, scholars working on problems of individual and collective identity have sought to demonstrate that the assumptions of the constructivist program, or paradigm, hold, and that those who have held or still hold primordialist or essentialist expectations and assumptions are wrong, usually laughably wrong. But for most students of identitarian politics and conflict, the real challenge is operationalizing the categories of constructivism so that progressive problem shifts can move attention beyond the hard core of constructivist assumptions toward the testing of interesting, knowledge producing propositions.

In other words, it is widely agreed that political and politicizable identities are not stamped "primordially" on groups or individuals within groups, that the translation of observable homogeneity among individuals into collective perceptions, goals, and behavior requires explanation, that identities are malleable, tradable, and deployable, that groups and individuals have repertoires of identities that are activated differentially in response to changing incentive structures, and that some actors can have disproportionate influence on patterns in the activation or consolidation of particular identities at the group level. But the constructivist research program that has established these assumptions as nearly hegemonic

within a large scholarly community has been in a slump. It has been too satisfied with its ability to discredit primordialist approaches, and not sufficiently committed to answering questions that primordialists could not ask. This ritualized beating of primordialist and essentialist dead horses can be explained in part by lack of theoretical imagination, but also in part because of the difficulties of gathering data suitable for the categories constructivist theory suggests as crucial.

Primordialism, for all its faults, had the virtue that once people were sorted into the proper "zoological" groups, with their essential characteristics divined, confident predictions could be made about the preferences, perceptions, and behavior of their members without actually examining or observing them. Constructivists, on the other hand, must somehow probe the multiplicity of identities available to individuals, the range of "identity projects" available within a population or across overlapping or intermingled populations, and the relationship of those identities and projects to changeable sets of preferences and changeable institutional circumstances. The data gathering problems created by the theory are compounded when the researcher's interests are directed toward exotic, logistically inconvenient, or even dangerous field sites.

As work done by many intrepid and theoretically sophisticated field researchers shows, it is possible to gather and analyze data relevant to constructivist images of how people trade, instrumentalize, or contextualize their politically relevant identities. It is even possible to discover or arrange natural experiments to use available data

to explore the plausibility of certain basic expectations of the overall constructivist posture. Although the overwhelming majority of this research has been focused on simply illustrating or demonstrating the constructed nature, the constituted aspect, of political or cultural identity, some have managed more than this, including some members of the Laboratory in Comparative Ethnic Processes.

My strategy, however, in collaboration with a number of researchers at the University of Pennsylvania, has been radically different. Instead of field research in settings where information about actual identities and projects of real people might be gathered, and then their behavior measured and compared under the conveniently varying conditions of a natural experiment, we have sought to exploit the power of computer simulation and agent-based modeling. We use these tools to produce hundreds, even thousands, of virtual histories of politics arranged as simply as possible to incorporate the formalized tenets of constructivist theory. By adjusting parameter settings and then producing large numbers of simulation runs, or virtual histories, we can then explore the relative robustness of various factors claimed to be important by constructivism in the crystallization, mobilization, and transformation of collective identity. Such techniques let us address key questions implicit in the constructivist research program, but which have seldom if ever been posed, let alone answered.

For example:

- If we understand that identities are multiple, how multiple are they, and what difference does it make if groups vary on this dimension?

- If we understand that identities are responsive to incentives, how responsive are they, and what difference does it make if incentives fluctuate rapidly or slowly, or within a narrow or wide range?
- If we understand that identities can be institutionalized, how does that occur and under what conditions of incentive fluctuation, repertoire size, and leadership, can institutionalized identities be stripped of their status?
- If we understand that political organization can produce or consolidate identities, how much organization does it take to stabilize or protect a dominant identity, under what conditions of repertoire size, environmental turbulence, polity size, etc.?
- If we understand that entrepreneurs of culture or identity can conjure new imagined communities, how different are they from non-entrepreneurial identity-deployers, how many of them are necessary, and how many make a significant contribution to the behavior of the group as a whole?

In our work with the Agent-Based Identity Repertoire (ABIR) model we have tried to create virtual worlds inhabited by agents operating according to simple algorithms—recipes for responding to their world that are not only consistent with the cognitive limitations we know are true of human beings, but which reflect the key propositions of constructivist theory, as adumbrated above. Thus in these artificial worlds, no aspect of collective organization or collective identity is present other than that which arises out of the

complex processes emerging from repeated inter-agent interactions over time.

These worlds, known as landscapes, are two-dimensional spaces inhabited by square shaped agents. Each square shaped agent interacts in each time period with agents in its “Moore neighborhood” (the eight agents who touch it on its sides and corners). Each agent appears as a particular color, to which a number is assigned labeling this, its currently “activated” identity. At the beginning of a “run” (which will be a “history” of the polity) the landscape can be “reseeded,” randomizing both the distribution of activated identities and that of subscribed identities. Each agent is endowed, as people are in constructivist models, with a repertoire of identities. The identities in each agent’s repertoire, including its activated identity, comprise a subset of the total number of identities present in the repertoire of all agents in the landscape. These identities, and even identities available in the political space but not, initially present in a particular agent’s repertoire, can be activated by that agent or brought into its repertoire and then activated. The simple algorithms which determine how and when an identity is included or extruded from an agent’s repertoire or activated by an agent were designed to correspond as closely as possible to the assumptions of constructivist theory.

Space does not here permit explanation and exemplification of how ABIR can be used to address the various questions listed above. Suffice it to say that characteristics of agents and the space they inhabit, such as size of agent repertoire, the presence of identity entrepreneurs and innovators, the

sensitivity of agents to changing incentive structures, the persuasive influence of agents, the presence of apathetic or fanatic agents, the size of the space, the shape of borders inside and around the space, the volatility and riskiness of the environment, and other constructively relevant variables can be manipulated into producing large numbers of statistically analyzable histories. To download an executable version of the ABIR program, along with a manual explaining its use, see <http://www.polisci.upenn.edu/profileil.html>.<sup>6</sup>

Among the topics we have focused attention on so far has been the implications of individual agent repertoire size for patterns of collective identity consolidation. We measure rates of collective identity consolidation, or “aggregation,” by using a modified Herfindahl Index score to report the “market share” possessed by different identities. We have found strong evidence to suggest a curvilinear relationship between repertoire size and tendencies for polities to organize around a smaller number of more “popular” identities. Polities comprised of agents with very small repertoires tend to produce “atomized” histories, with low aggregation patterns, where most or all identities available in the polity as a whole are represented by small but significant numbers of activated agents. As the size of repertoires at the agent (micro) level increases, aggregation levels increase. That is, the number of identities prominently displayed within the landscape by clusters of agents activated on those identities decreases, while the portion of the landscape these identities occupy increases. This effect occurs as a result of enhanced opportunities

available to agents with overlapping repertoires to discover mutually profitable identities for activation. However, as the size of agent repertoires gets very large, and begins to approach the number of identities available in the polity as a whole, the “aggregation” rate falls. We trace this pattern to the rapidity with which regions occupied by agents with very large repertoires organize themselves into homogeneous blocs whose bulk and uniformity impede response to a changing incentive structure.

Of particular moment, we believe, is our finding that a relatively small departure from the principle of random distribution of identities into the “subscriptions” (non-activated repertoires) of agents dramatically increases the likelihood of cascade effects when the size of agent repertoires are neither very large nor very small. This non-intuitive result suggests that the presence of some racist or otherwise exclusivist identities, identities which resist cohabitation in the same repertoire with other identities present in the polity, can greatly increase the likelihood of domination of the political space by one of the “inclusivist” identities. In other words, a little racism in a society may make it more likely that power ends up concentrated in the hands of a large number of citizens activated on a smaller number of non-racist identities.

We have also found that patterns of tension, diversity, and aggregation in a polity are significantly affected by changing the volatility and/or the range within which the environment of the polity changes. Generally speaking, the more volatile, and the more risky (the greater the possible change in the incentive structure when change does take place), the

less tension, the less diversity, and the more aggregation will be observed, until the repertoire size increases considerably, at which point these measurements tend to reverse themselves. However, these relationships interact with repertoire size and the presence or absence of “entrepreneurial” agents. For example, under “turbulent” conditions and when repertoire size is small, the presence of “entrepreneurs” or “opinion leaders” encourages aggregation, lowers tension levels, and reduces very high levels of diversity (identities remaining in the landscape activated by at least one per cent of the population). However, as repertoire size increases, under turbulent conditions, these “mobilizer” agents “flip” their impact to prevent aggregation levels from becoming extremely high, encourage a certain amount of tension (opportunities for adaptation), and raise diversity levels (Lustick and Miodownik 2000).

Some of our current work focuses on the institutionalization of identity and the operationalization of the notion of a “consolidation threshold” beyond which an identity prominent in a polity becomes an order of magnitude more resistant to losing its “dominant” status despite streams of intensely negative signals from the environment.<sup>7</sup> We consider that this work is relevant both to processes of identity crystallization and institutionalization within individuals as well as within polities. Additional studies are now being done with ABIR to study learning as an emergent property of polities and to study the relationship of globalization to patterns of identitarian resurgence.<sup>8</sup>

#### Symposium Footnotes

<sup>1</sup> A bibliography of the works cited in this symposium is available on the Newsletter’s website.

<sup>2</sup> Geertz’s primordialism in the statements that I quoted is actually much more flexible than I remembered: it allows for a considerable degree of multiplicity and fluidity of ethnic identities. For instance, Geertz (1963, pp. 153-54) writes about gradual shifts of primordial loyalties in non-Western societies from smaller groups to larger groups and the fact that, as a result, individuals may have simultaneous attachments to more than one group. He describes this process as “a progressive extension of the sense of primordial similarity and difference generated from the direct and protracted encounter of culturally diverse groups in local contexts to more broadly defined groups of a similar sort interacting with the framework of the entire national society”; a couple of his examples are “becoming an Outer Islander in addition to a Minangkabau” in Indonesia, and “a Yoruba rather than only an Egna” in Nigeria.

<sup>3</sup> These figures were calculated from data on religious demographics and language use reported in Hunter (1997).

<sup>4</sup> The variables “religion” and “caste” are then subcategorized so that we can tell which religious groups or castes were involved. My hope in the future is to try to deal with the question of whether particular caste or religious identities are prone to higher levels of violence than others.

<sup>5</sup> This explanation confronts large empirical anomalies, however. Specifically, it seems to predict that immigrants should compromise with all groups in their new homelands, while in fact settlers compromise with each other but deal harshly with indigenous people.

<sup>6</sup> For published work reporting results of research conducted so far see Lustick 2000, available at <http://jasss.soc.surrey.ac.uk/3/1>; and Lustick and Miodownik 2000. Both these articles contain illustrative screenshots of the model.

<sup>7</sup> For preliminary results see Lustick, Miodownik, and Philbrick 2000, available at <http://pro.harvard.edu>

<sup>8</sup> Roy Eidelson of the Solomon Asch Center for Study of Ethnopolitical Conflict at the University of Pennsylvania and Maurits van der Veen, a Postdoctoral Fellow at the Christopher Browne Center for International Politics are working on these projects in our laboratory at the University of Pennsylvania. Important support for our efforts has been received from the Carnegie Corporation of New York. ❖

# Controversies

**On the "Call for Papers" for Division 11, Comparative Politics, for APSA 2001**

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After APSA 2000, I gather that comparativists are already looking forward to the 2001 Congress in San Francisco. I am sure, too, that all of us are grateful to Professor Barbara Geddes for having taken on the responsibility of chairing the Comparative Politics Division of the 2001 Congress. However, I was surprised and worried to read, in page 220 of the 2000 Program, Geddes' Call for Papers for this event.

Geddes devotes more than half of this text to asserting that comparativists were unable to predict events such as the demise of authoritarian rule, the abandonment of state-led development policies, the adoption of a "greater market orientation" by many countries, and the collapse of the "Soviet empire." This is simply not true in relation to these topics, including the oft-repeated failure to predict the fall of Communism; indeed, well before this event several authors discussed the severe flaws and tensions of communism in ways that at the very least anticipated its fragility. Geddes' assertion is likewise not true regarding topics she does not mention but clearly implies in the sweeping tone she adopts in deprecating the contributions of the field. I refer, among others, to studies, started at least in the 1970s, on social and political revolutions, on historical/

comparative factors in the emergence of democracy and authoritarianism, on patterns of incorporation and exclusion of the popular sector, on changes in value sets in advanced capitalist countries, on processes of European unification, on neocorporatism and varieties of capitalism, on the changes and crisis of the welfare state, on the transformations of parties and party systems in old and new democracies, on the characteristics and dynamics of various kinds of regimes, and on the breakdown of democracy. Geddes' assertion is also untrue in relation to events that simply have not occurred, such as her remark that "states [in Western Europe] were voluntarily giving up control over national policy." Worse, Geddes ratifies her denial of several decades of fruitful comparative research with the assertion that "comparativists ..., even today, could more persuasively explain why [these events] should not have happened than why they did." Apparently, the field has not been able to explain anything! In contrast to other supposedly flourishing areas of political science, theoretically sound and endowed with powerful predictive tools, the field of comparative politics that Geddes is convening seems to have suffered a severe problem of adverse selection of its practitioners.

Furthermore, Geddes commits an obvious non sequitur when implying that the disappearance of authoritarian regimes invalidates the attempts to explain their emergence and functioning. With this strange logic, theories about fascism, communism, empires, or for that matter history

itself, or the study of dinosaurs, would be invalidated by the fact that their subject matter does not exist any longer. In addition, it entails grave injustice to the pertinent literature, both on the breakdown of democratic regimes and on authoritarian rule, to ignore that it stressed the tensions and the ultimate fragility of the authoritarian regimes that emerged in Latin America and elsewhere in the 1960s and 1970s. Geddes may not want to call this view a 'prediction' (a point I do not care to dispute here), but it is demonstrable, as attested by an abundant literature, that this same view led to studies on the transition from these regimes well before their transitions had begun. To assert, as Geddes does quoting Hirschman, that the studies of Latin American authoritarianism were focused on "their majestic inevitability and perhaps permanence" means serious ignorance of a literature that, as somebody who has done good part of her work on this region, she has the professional obligation of acknowledging, if not for its intellectual merit for what this literature explicitly and repeatedly stated.

In sum, this Call for Papers entails the sweeping denial of the contributions of comparativists on the topics it mentions and, implicitly, as noted, on many others. Of course, much remains to be done in the study of comparative politics, both in relation to the past and, as Geddes puts it, concerning "the great changes that have recently occurred." But the way to induce these contributions is not the outright denial of past contributions, but the creative and con-

structive invitation to build upon them. In any case, from what standpoint external and superior to the plodding field of comparative politics, or from which crowning theoretical achievements (predictions included), can such assertions be justified? Actually, this Call for Papers is a summary restatement of the view articulated by Geddes some time ago, to the effect that in order to overcome the precarious "sandcastles" that according to her comparative politics has built, only rational choice approaches will do. (1) For various reasons—including the fact that I consider rational choice, with some limitations and caveats, a very useful analytical tool—this is not the place to discuss this matter. Here I limit myself to comment that the role of Division Chair demands an open-minded attitude to the approaches that do exist in a given field. This is particularly true when, as it is the case of comparative politics, except for some very partisan observers there is wide agreement that those approaches retain considerable usefulness.

I am persuaded that most of us who have worked on comparative politics for some time know that it would be fair to recognize both the contributions and the positive value of the variety of approaches employed in this field. But our younger colleagues and our students may need to be told about the spirit of openness, of pluralism of approaches and methods, of experimentation, and of respectful dialogue and collaboration—across approaches and, indeed, across regions of the world—that has characterized our field. Since I believe that this spirit has been, and should continue being, a great asset of our field, I hope that, despite this unfortunate Call

for Papers, Geddes will make her decisions as chair of this Division in a way that is consistent with that same spirit. I also hope that future Division Chairs will convene their colleagues with the open and constructive spirit that the present Call for Papers lacks. ♦

#### FOOTNOTE.

1. Barbara Geddes, "Paradigms and Sand Castles in the Comparative Politics of Developing Areas," in William Crotty, ed., *Political Science: Looking to the Future. Comparative Politics, Policy, and International Relations. Volume Two* (Evanston: Northwestern University Press, 1991), p.45-75.

## Reply to O'Donnell

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I am sorry that Professor O'Donnell finds my Call for Papers so offensive, especially since I have always been an admirer of his work. I am mystified by his interpretation of the Call, however. To say that we failed to predict the sweeping changes that have occurred in the world does not imply a denial of the value of vast amounts of research in the comparative field. Nor does it imply that "only rational choice will do."

At a roundtable on the contributions and limitations of rational choice in the study of Latin American politics at the 2000 APSA meetings, I expressed the opinion that it is time to move beyond debates over rational choice. Those of us who use rational choice learn continuously from those who do not, and many of those who find the approach uncongenial have nevertheless been

influenced by the insights it has brought to light. The interaction among approaches has been fruitful if occasionally acrimonious, and I expect this year's APSA panels to continue the tradition of multiple competing approaches.

Even if I were as unreservedly committed to the rational choice approach as Professor O'Donnell believes me to be, I would not consider it appropriate for the section organizer to impose her own tastes on the APSA Program for the comparative field, and I would not attempt to do so. My Call for Papers is reprinted below so that readers may judge it for themselves.

"It was the best of times, it was the worst of times, it was the age of wisdom, it was the age of foolishness...." So begins *A Tale of Two Cities*, Charles Dickens' novel set during the French Revolution. We also are living during a period of great changes in the world and compelling challenges to received wisdoms. At precisely the moment when transitions to authoritarianism had, in Albert Hirschman's words, "been fully explained by a variety of converging approaches and [were] therefore understood in [their] majestic inevitability and perhaps even permanence," democratization swept through much of the world. In a second equally unexpected development, many governments began to abandon their decades long commitment to state-led development strategies in favor of greater market orientation. Meanwhile in Western Europe, the cradle of the nation-state, states were voluntarily giving up national control over policy. On top of everything else, the Soviet empire collapsed. Though comparativists have greeted most of these events with delight, they did not predict

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# Book Review

**Charles Ragin's  
Fuzzy-Set Social Science  
(University of Chicago  
Press, 2000)**

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Charles Ragin's book is a useful push towards bringing fuzzy sets into the view of mainstream social scientists. Fuzzy sets and multivalent logics have themselves been around for a while, having been reinvented several times by different people before finally being articulated clearly by Lotfi Asker Zadeh in 1965. Zadeh himself thought that fuzzy sets would play a role in social scientific analysis, but, except for some isolated work, e.g., Smithson (1987), Sanjian (1988), Taber (1992), or Seitz (1994), there have been few extant uses in real empirical research, though there has been the occasional review article, e.g., Cioffi-Revilla (1981). (A search in all economics, political science, and sociology journals in Jstor using the keyword "fuzzy set" turned up a total of twelve entries. In several cases, the term turned up in the citations only—no substantive use was made of fuzzy sets. I cited the examples from political science. Interestingly enough, *none* of these works was cited in this book, though two of the three—Cioffi-Revilla, Sanjian, and Taber—were published in a mainstream political science journal, *AJPS* or *APSR*, and the Seitz piece is published in an important

philosophy of science journal, *Synthese*.) Systems scientists have made attempts at applying fuzzy sets to social science, but, while their understanding of fuzzy sets is deep, their understanding of social science is often not especially so, and their work has had little impact. Nevertheless, the approach has a lot to offer the working social scientist, so it is very useful to see a long-awaited book written by as prominent a scholar as Ragin raising attention for the use of fuzzy set theory in social research.

The introduction of the book sets out the problems in conventional research practices as Ragin sees them, and is a useful place to start. First and foremost is a lack of connection between theory and methods used for testing. Theory is most often expressed in logical or, equivalently, set theoretic terms, but quantitative testing is done using methods that do not deal with the statements. For instance, Barrington Moore's classic theory of democratization, put rather schematically, states that unless a country has a strong bourgeoisie, it will not be democratic. This is essentially a set-theoretic statement, which is to say, if a country is not in the set of countries with strong bourgeoisies, it will not be in the set of democracies. Putting it in a completely categorical fashion requires that one code cases dichotomously, strong or not, and examine the pattern of cross-tabulation between cases. Are the strong bourgeoisie cases indeed democratic?

There are two problems here. First, correlations are symmetric in that, if X and Y are correlated,

it doesn't matter which way you calculated it, but it is highly desirable to have an asymmetric measure of relationship, which parallels more closely what we think of as causality. Clearly it means something quite a bit different to say that, to continue our Barrington Moore example, all democracies have strong bourgeoisies—a statement about subset-hood that does not imply the converse—than democracy is correlated with a strong bourgeoisie, which is symmetric and provides no means of sorting out which comes first. Furthermore, a triangular pattern of cross tabulation as per Figure 1 will tend to have a lower correlation between the two variables compared to the relationship in Figure 2. (The correlation coefficient here would be  $(ad - bc)/[(a+b)(c+d)(a+d)(b+c)]^{1/2}$ , where a is the number of cases in the upper left cell, b is the number of cases in the upper right cell, c is the number of cases in the lower left cell, and d is the number of cases in the lower right cell, that is, coefficient phi. Assuming a fixed number of cases, in Figure 2 the denominator is smaller than the denominator of Figure 1 because b is larger, which reduces the overall correlation.) The relationship in Figure 1 is not easily interpretable in terms of linear relationships, but can be interpreted in terms of subset-hood; here Democracy is a subset of Strong Bourgeoisie. (I should note that there has been at least one measure of subset-hood for over five decades: the Guttman scale's coefficient of reproducibility. The triangular scatter plot discussed by Ragin is, in fact, exactly Guttman's

condition for scalability of two items. See Smithson (1987), pp. 94-95, for more details.) Once one gets to subset-hood, it is possible to make nice connections with necessity and sufficiency of explanations, which Ragin does in some detail for both crisp sets and, in the second half of the book, fuzzy sets.

**Figure 1: Example of a Subset Relationship**

	Democracy	No Democracy
<b>Strong Bourgeoisie</b>	+	+
<b>Weak Bourgeoisie</b>	-	+

**Figure 2: Example of a Linear Relationship**

	Democracy	No Democracy
<b>Strong Bourgeoisie</b>	+	-
<b>Weak Bourgeoisie</b>	-	+

The second problem with the usual strategy is that, unfortunately, it can be very difficult to code things dichotomously. Results can then depend very strongly on arbitrary coding decisions and a lot of useful information about how cases differ from each other is lost in dichotomous coding. One would be tempted to use something polytomous. But, as Ragin points out, this can really miss the point, depending on the method one uses. Correlation (or regression) compares cases to the average, but the average may not be a particularly meaningful reference point vis a vis one's theory. Having a method that assesses the set theoretic claims directly in a way that is sensitive to gradations of membership seems better.

This is precisely what fuzzy sets can provide.

After the lack of connection between theory and method, the second issue is that conventional approaches to research tend not to be sensitive to context. Researchers use variables and attempt to assess independent causal impact based on marginal effects. It is quite common to see multiple regression models that are simple additive structures of a dozen or so "independent" variables, or rather somewhat thin, typically easily obtained proxy measures that may or may not measure the concept in question. There are a number of problems with this sort of model, but the one to single out here is that it pays no attention to combinations of variables; values of variables are assumed to be perfectly substitutable and the impact is the same across cases; that is

cases are homogeneous. From a perspective of "surface modeling" this might give reasonable predictions for relatively cheap cost, but from the perspective of "deep modeling," where one wants to understand causality, not simply find convenient predictors for variables in question, assuming that variables have homogeneous impact can be quite misleading.

The usual framework for causal assessment, adapted from the experimental methods developed by Neyman, Pearson, Fisher, and others in the context of biological experimentation, as useful as they are in their context, do not really do justice to the problems of social scientific causal assessment, especially in an observational context. As I mentioned

before, Ragin spends a great deal of effort in the book developing methods, using his Qualitative Comparative Analysis (QCA) methodology as a basis and the deep connection between set theory and logic, to assess necessity and sufficiency of causes. These methods have been implemented in a computer program, FS/QCA. Interested readers would do well to download this program from Ragin's web page and try things themselves, replicating Ragin's examples from chapter 10 or, better yet, trying something on their own problems. This is some of the more interesting material to come around in a while. The main reservation I have is that, like QCA, I wonder how useful it would be in a longitudinal context. I also worry that one might be tempted to think that because we have a quantitative measure that can be interpreted in terms of necessity and sufficiency, we therefore have obtained the holy grail of quantitative social science: a theory- and value-neutral procedure for assessing causality. This is no more true than it ever was.

Fuzzy sets get you more than just this. In addition, as chapters 6 and 7 discuss, fuzzy set theory provides a number of operations that allow researchers to model quite complicated concepts that are closer to the verbal statements we make when theorizing. For instance, fuzzy set concentration allows one to model the linguistic statement "very X" where X is a particular fuzzy set. (There is some dispute among cognitive scientists over whether the fuzzy linguistic hedges actually model real world linguistic hedges. Insofar as the hedges are used in a precise scientific context with well-defined and carefully examined concepts,

this makes little difference.) However, Ragin's coverage of the possibilities of fuzzy set theory leaves much to be desired. There is no mention of possibility theory, fuzzy relation models, expert systems, alternative fuzzy operators, or a whole host of other important topics. Nor is there a discussion of the various issues and difficulties one faces in applying fuzzy set theory that are key for more sophisticated applications. There isn't even an adequate literature review, as I mentioned in the introductory paragraph. As a gateway to the literature on fuzzy sets for the interested researcher, this book is deficient.

For all Ragin's advocacy of "fuzziness" he sets up a rather "crisp" picture of variable-oriented vs. case-oriented research, perhaps rather too sharply contrasted. There are many quantitative approaches that are sensitive to configurations of properties and/or set membership in various ways: canonical correlation, multidimensional scaling, correspondence analysis, latent class analysis, and that old horse Guttman scaling, to name a few. Simply put, though it often may seem this way, quantitative analysis is *not* simply multiple regression. More attention—at least acknowledgement of this—would be useful.

Another reservation I have is that many systematic methods for assigning membership function values are ignored. Theoretical relevance is certainly essential. However, membership function assignment is a difficult business and it is unwise to throw away tools, though that seems to be what Ragin's advice is: "In general, however, it is better for social scientists to base their fuzzy membership scores on theoretical and

substantive knowledge as much as possible and to avoid surrendering too much scholarly authority to computer algorithms." (p. 170) Or at least it could be taken as such by the unwary. Psychometricians, for one, have spent a lot of time and effort on methods for metrizing categorical or ordinal data, but the only formal measurement method mentioned is factor analysis, which suffers from the problems discussed in the book, being based on linear decomposition of correlation matrices. A look through a current textbook on fuzzy sets for engineers (George A. Klir & Bo Yuan (1995), *Fuzzy Sets and Fuzzy Logic*, Englewood Cliffs, NJ: Prentice Hall) shows the authors paying some attention to this issue, but, again, this is not even cited. Without a counterbalancing check on social scientists' exercise of theoretical prerogative, it seems to me that there is great potential for ad hoc assignment of membership functions, leading them to "mean what scholars want them to mean," to paraphrase Lewis Carroll. And there is a further problem. Even if one uses several investigator-provided theoretical ratings on indicators, often it is too difficult a task for an investigator to move from indicators to a scale in a consistent basis, in which case it is nice to have a box of tools for aggregating several scales into one membership function.

The book is a useful mark in the road and discusses a broad range of issues, but one needs a lot more than just this book to make use of fuzzy set theory in one's research. Read the Ragin book as an introduction, but take a look at Smithson and other places to dig deeper. ❖

Cioffi-Revilla, Claudio (1981), "Fuzzy Sets and Models of International Relations," *AJPS* 25(1), pp. 129-159.

Sanjian, Gregory S. (1988), "Fuzzy Set Theory and U.S. Arms Transfers: Modeling the Decision-Making Process," *AJPS* 32(4), pp. 1018-1046.

Seitz, Steven Thomas (1994), "Apollo's Oracle: Strategizing for Peace," *Synthese* 100, pp. 461-495.

Smithson, Michael (1987), *Fuzzy Set Analysis for Behavioral and Social Science*, New York: Springer-Verlag.

Taber, Charles S. (1992), "POLI: An Expert System Model of U. S. Foreign Policy Belief Systems," *APSR*, 86(4), pp. 888-904.

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**Sage Award for Best Paper in Comparative Politics presented in the 2000 APSA Meetings:**

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fit from ignoring much of what we know about the world to better develop an understanding of the fundamental relationships in play.

I would wager that we will eventually have a unified theory that is sufficiently powerful to be adopted by almost everyone. Moreover, I would guess that the future unified theory will be a generalization of rational choice to encompass more information regarding human motivation and weaker assumptions regarding people's ability to calculate optimal strategies. Others, whose opinions I respect, have very different views of what the future unified theory will look like or whether a useful unified theory will ever exist. In the meantime, most of us will continue to use what we perceive to be the best theoretical tool for the particular job we face. The only way to convince others that our theory is the best candidate for the status of the "theory of everything" is to demonstrate that its explanatory power exceeds that of other approaches. And the only way to know whether a unified theory is possible in comparative politics is to wait and see if rational choice or any other candidate can be developed into a sufficiently powerful set of explanatory tools that few will want to do research using anything else.❖

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them and, even today, could more persuasively explain why they should not have happened than why they did.

This ferment in the world has shaped the research agenda in the comparative field. Proposals for research in two areas will be especially welcome for the 2001 APSA meetings: research aimed at explaining the great changes that have recently occurred; and research that seeks to build an understanding of contemporary politics and political economy in new democracies, new states, and new transnational unions. Proposals should include a brief discussion of research strategy and evidence along with a clear statement of the research question and a summary of the argument tentatively proposed.

Proposals for novel variations on the standard panel format will get serious attention. ❖

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