

## What Has Comparative Politics Accomplished?

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The following feature is excerpted from the forthcoming volume, *Passion, Craft, and Method in Comparative Politics*, by Gerardo L. Munck and Richard Snyder. The authors conducted in-depth interviews with fifteen leading scholars in the field of comparative politics: Gabriel A. Almond, Robert H. Bates, David Collier, Robert A. Dahl, Samuel P. Huntington, David D. Laitin, Arend Lijphart, Juan J. Linz, Barrington Moore, Jr., Guillermo O'Donnell, Adam Przeworski, Philippe C. Schmitter, James C. Scott, Theda Skocpol, and Alfred Stepan. These scholars reflect on their intellectual formation, their major works and ideas, the nuts and bolts of the research process, their relationships with colleagues, collaborators and students, and the evolution of the field. The material excerpted here addresses the achievements and shortcomings of comparative politics and is taken from four of the interviews.

### ROBERT A. DAHL

**Q: What are some of the main achievements of comparative politics over the last 50 years?**

A: Over the last 50 years, there has been an enormous gain in the quality and quantity of knowledge that we have in the field of comparative politics. This is a very positive and impressive change. I don't know that it's a bigger change than has occurred in other branches of political science, but it might well be, especially since comparative politics started with a rather narrow base. When I was a graduate student at Yale in the 1930s, Herman Finer's *The Theory and Practice of Modern Government* (1932) was the main text in comparative government courses. That text concentrated on the United States, Britain, France, with perhaps a little bit on fascism and the Soviet Union. At that time, comparative government was a very narrow field with a primarily historical, descriptive, and institutional content. There was a lot of valuable information there, but it was not theoretical, at least not in the sense of theory as we mean it today.

Overall, the field was very Eurocentric. There was no study of Japan or China, for example. There may have been some people who wanted to study the Soviet Union. But until the outbreak of World War II, our horizons were really quite limited. The big thing going on in the world was the New Deal. There were very few people in the field who had mastered the languages, even Russian, that were required in order to understand non-European countries. Even Latin America - a rich treasure house of experience nearby - was not within our ambit. I don't think we studied Canada. It was all very parochial.

The expansion outward of the study of comparative politics beyond a handful of European cases, the growth of often quite good world-wide data, and the development of methodologies and analytical techniques have fundamentally altered the field in a positive way. We know a lot more about political parties, constitutions, and about broader things like regime breakdowns and transitions. Fifty years ago we knew almost nothing about these things. We know so much more about electoral systems, their consequences, and how they function than we did only a few years ago. The growth of knowledge about these important issues is heartening and positive. I don't want to exaggerate and say we are anywhere near achieving a final, conclusive body of knowledge; we never will be, but we know so much more. I think the study of comparative politics and comparative democracies, specifically, might be the most promising part of political science today.

**Q: What are some of your major disappointments with the field of comparative politics?**

A: I think it's appalling that at this late date we are still struggling with how to conceptualize and measure democracy. I find the continuing debates about what we mean by democracy - I mean this and he means that - depressing.

**Q: Why do you think it has taken so long to come up with a satisfactory definition of democracy?**

A: Part of the reason is that a satisfactory definition has to respect the history of the term. You don't want a definition of democracy that makes the Greek city-state undemocratic. You have to accommodate that. At

the same time that a satisfactory definition of democracy has to respect the history of the term, it also has to be able to accommodate the evolution in its meaning. And a satisfactory definition also has to be formulated in a way that allows you to measure it. This requires judgments and rankings

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based on judgments. For example, you need to judge how free speech is in Peru. This is not like reading a scale on a thermometer, but with a large degree of observer agreement on such judgments, you can put some trust in them. These are all demanding requirements that are rarely met.

**Q: Do you have any other disappointments with the field?**

A: I’m enormously disappointed that the study of power and the conceptualization of power have made no progress that I can detect since Harold Lasswell and Abraham Kaplan’s book in 1950, Jim March’s work (1955, 1956, 1957) and my early efforts (Dahl, 1968) Power is such a central concept, and Jim March and I had hoped a vocabulary would evolve that would allow for observation, comparison, and the accumulation of information. We also hoped a precise and discriminating language for studying power would evolve, along the lines of what Lasswell had tried to develop and what I tried to develop

for political analysis (Dahl, 1963). These expectations turned out to be highly optimistic. Now, fifty years later, I see people use the word and concept “power” as if we were back where we started. Even elementary distinctions going back to Max Weber - such as the distinction between power and authority, or legitimate power - seem to have been forgotten. So perhaps we’ve not only failed to progress in the study of power, we’ve actually gone into reverse.

Also, very few people study power today. I don’t know what explains this. Perhaps the requirements for studying power in a way that we would now regard as methodologically sound and reasonable outstrip our capacities for definition and measurement. Maybe the problem is that we don’t have good ways of measuring power, so the people who would be likely to study it know that the methodological requirements are just too daunting.

**JUAN J. LINZ**

**Q: Several scholars (Lindblom 1997, Geddes 2003) have recently questioned whether comparative politics has generated cumulative knowledge? What do you think about this issue?**

A: I think there are several different areas of research in which we have quite a lot of learning and cumulative work. One example of learning can be seen in the research on consociational democracies by Arend Lijphart (1968, 1977) and others That work persuasively challenged the old prevailing notion that a majoritarian, two-party model of democratic politics worked far better than a multi-party system. Now we know that a number of democracies with multi-party sys-

tems have actually worked very well. That’s an insight we didn’t have when some of the literature on political parties started. Similarly, the research on corporatism, starting with Phillippe Schmitter (1979) and Gerhard Lehbruch (1982), taught us that a close integration of interest group politics and political parties was not necessarily a bad thing for democracy. I think the comparative literature on transitions to democracy has taught us something about how to make a transition possible. In particular, we have learned that transitions take place within the institutional framework of the previous regime, can be negotiated, and are not necessarily violent breaks with the past. To take another example, work by people like Giovanni Sartori (1994), Rein Taagepera and Matthew Shugart (Taagepera and Shugart 1989, Shugart and Carey 1992) on the consequences of different electoral laws has also generated lots of solid knowledge. Finally, comparative work on elections using survey research has produced a lot of cumulative knowledge about the relationship between social variables, like class and religion, and voting behavior. The problem is that our knowledge about something like voting behavior is cumulative only until you have a political earthquake. Then some of the relationships among the variables no longer hold. For example, the library I have in my basement on Italian politics and voting was very cumulative from 1948 to the 1990s. But then the Christian Democratic Party disintegrated, Berlusconi and the Northern League emerged, and the whole party system changed. This means that to understand Italian elections you have to start from scratch, well, not from scratch, but you have to start anew. Previous research cannot be the only basis for analysis. By

creating these discontinuities, Italian politicians were being cruel in a sense to the social scientists that had spent decades working on Italian parties and elections.

More generally, all the knowledge generated by research on voting behavior in advanced industrial democracies is getting dated. This is partly because the working class, which used to represent some 30 to 40 percent of the population, has shrunk dramatically in many places. The old linkages among variables like working class identification, trade union membership, and participation in labor, social democratic, or communist parties have been weakened. And you increasingly find a much more homogenized, "middle class society" in these countries. As a result, the old loyalties to parties have eroded. So, a worker who, in the past, would have said "I am a worker, therefore I am a union member, and therefore I have to vote social democratic or communist" today might say, "I am a worker, but I have a summer home on the Mediterranean coast which I rent to tourists, and the social democrats propose to raise taxes on my second house." So, that person, even though he is still a worker, may vote against the party he would have voted for in the past. And voters today are much freer in some ways. For a long time, the Italian voter saw the Christian Democrats as his protection against the threat of the communists coming to power. So, no matter what misgivings he may have had about the Christian Democrats and the corruption that characterized their governments, he voted Christian Democrat.

Today, no parties are seen as a serious threat, and Italian voters thus feel much freer to vote for whichever party

they think best fits their interests. This makes it infinitely more difficult to predict voting behavior in Italy. To take another example, decades ago, whenever I met a Dutchman, I only had to ask him two or three questions - Are you a Catholic, a Calvinist, or a non-believer? What's your occupation? - and I knew how he would be voting, because 90 percent of the voters with certain social characteristics voted for a particular party. That's not the case any more, which obviously makes the study of political parties

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more difficult. All these types of changes set limits on our ability to generate cumulative knowledge about politics.

***Q: What are some of the areas of research in comparative politics where you think our knowledge is especially limited and in need of improvement?***

A: I think we know much too little about political leadership and the quality of political elites. We know that political elites usually have higher education, come from a certain background, know foreign languages, have studied abroad, and so on. But we do not know why some leaders are more creative and more committed than others, and why some leaders are real crooks. As Schumpeter (1942) notes at the very beginning of

his theory of democracy, in order to have a working democracy you need a pool of qualified people who are committed to public service. Why do some societies have these people but others don't? And why do some countries produce creative business elites? In today's *New York Times* there is a story about the Hyundai empire. Why did the Hyundai empire emerge in Korea and not in Argentina or somewhere else? These are things we don't know. The number of things we know so little about is startling. There is so much work to be done.

## **SAMUEL P. HUNTINGTON**

***Q: What are the most important achievements of the field of comparative politics?***

A: If you go back to the 1950s, scholars in comparative politics performed a very useful service in rapidly studying the politics of developing countries. Through World War II comparative politics really was nothing but the study of the major European powers and the United States. If you look at any textbook of comparative politics from back then, you have five chapters: one on the United States, one on Germany, one on France, one on Britain, and so forth - that's it. It seems to me that with all its shortcomings, the literature on political development that emerged in the 50s, 60s, and 70s was very broadening and constructive. Also, comparative politics has become more sophisticated in its methods of analysis; and I'm all in favor of sophistication and methodology when it is useful, and in many cases it is useful. We now have increasing quantitative data on political development and on political variables and non-political variables that are relevant to politics. For example,

Karl Deutsch back in the 1960s put together the *Handbook of Political and Social Indicators* (Russett et al., 1965) - which was a very useful compilation of data. Now more data are available, and Ronald Inglehart, with the World Values Survey, which has limitations stemming from the fact that he has to rely on some not terribly sophisticated polling outfits in Third World countries, has compiled a very useful source of quantitative informa-

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tion on the values of people around the world (Inglehart 2003).

**Q: I'm struck by the fact that you haven't mentioned any big theoretical contributions or breakthroughs as major achievements.**

A: Theories come and go. The subfield has gone through phases - back in the 50s structural-functionalism was the big thing - Gabriel Almond and James Coleman's *The Politics of the Developing Areas* (1960) for example. Theoretical frameworks like that come and go - they may be useful for a period of time. In responding to your question about the major accomplishments of the subfield, I was trying to think of things that made a more lasting improvement. There have certainly been a wide range of propositions - empirical generalizations - which have stood the test of time. One example is the posi-

tive relationship between economic development and democracy. Marty Lipset set it forth in a rather crude form back in the 1950s. It's been refined, and dozens of scholars have built on his initial article. But overall his proposition holds up. Propositions about the impact of social and economic change on social and economic equality have also been enduring. In my own work (Huntington, 1968), I showed - and other people have supported the same conclusion - that political instability is not a product of poverty, it's a product of people getting *out* of poverty. I think the work of Donald Horowitz (1985, 2001) on ethnic conflict is also a very considerable contribution. At the juncture between comparative politics and international relations the so-called "democratic peace" proposition was a major contribution which, despite all the debate about it, holds up pretty well.

**Q: What are the major shortcomings and failings of the subfield over the last 40 years or so?**

A: Political science - including the study of comparative politics - is heavily influenced by other disciplines. The principal external disciplinary influences change from time to time. During the past several years, economics has been dominant. Before that it was sociology, and certainly Almond and others were heavily influenced by Talcott Parsons. But even before that there was the whole psychological approach to politics - for example, somebody like Harold Lasswell in the 30s and 40s tried to look at politics in terms of its psychological dimensions. It seems to me it would be useful to go back and revisit that earlier work on psychological approaches to politics, in part because there's always the tendency

- in the laudable desire to produce generalizations - for people to look at issues where you have a large number of variables that you can study, compare, quantify, and so on. This tends to omit the decisive role which political leaders play. The serious study of political leadership has been a great deficiency in recent political science.

In addition, 30 or 40 years ago, there used to be a very considerable literature and a lot of work done on political socialization. It might be time to go back and study political socialization, which basically means how people's political values develop. A lot of the literature 30 or 40 years ago was looking at children, asking from where they get their ideas about politics, about political leaders, the presidency, and so on. We're in a very different era now, and it would be useful to go back and look at political socialization in that context but also in a broader context about how political values change. If culture is important and there are some cultural systems - systems of beliefs and attitudes - that are conducive to economic and political development and other cultural systems that are not, the next ques-

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tion is how do you go about changing a culture? How do you change people's political and social attitudes, beliefs, and assumptions? That's a very important question on which I don't think much relevant work has

been done. One can think of examples of things that do change values. A traumatic event will change people's values. For example, Germany and Japan in the 1930s were the two most militaristic countries in the world, but the trauma they suffered in World War II turned them into two of the most pacifist countries in the world. That was a real change in values. Economic development changes people's values. I think that's very clear and can be seen in Inglehart's work (1990, 1997) on the development of materialist values and the shift to post-materialist values. But if you want to change values in order to *produce* economic development, that work is not much help.

## ADAM PRZEWORSKI

**Q: If you look at where the field of comparative politics was 30 years ago and where we are now, what are the main things we have learned?**

A: Let me preface my answer with one caveat. I think some of the best research in comparative politics is done these days by economists, so I will include them in my answer. Daron Acemoglu and James Robinson, Alberto Alesina, Torsten Persson and Guido Tabellini, and many others do excellent work in comparative politics. They don't know enough about politics, particularly about institutions, but they address central questions and get provocative answers. With that inclusion, yes, I think there has been a tremendous accumulation of knowledge.

What have we learned? Ever since Maurice Duverger's (1964) and Douglas Rae's (1969) seminal books, we have learned a lot about the con-

sequences of electoral systems. Gary Cox's book, *Making Votes Count*, (1997) is the latest example of it. We know how the electoral systems interact with social cleavages to produce parties, how they affect the distributions of votes, and so on. We've learned a lot about coalition formation and cabinet formation; there is a formal and an empirical literature on these topics. We understand much more about the legislative process. We've learned a great deal very rapidly in the last few years about ethnic conflict and ethnic peace. We have learned that most of the time ethnic groups live together in peace, and perhaps we are beginning to understand some mechanisms that explain this finding. Finally, I think we understand much more about the processes of regime transitions. I could go on.

More broadly, one test of the advances we have made is that when a student raises a topic with me, most of the time I can say "okay, read this, read that, here is the literature that says this and that." On various topics, the conclusions do not converge. But at least there are bodies of literature on a variety of topics.

**Q: Are there any topics on which we have not made significant advances?**

A: We still do not know why and when people with guns obey people without them: the determinants of civilian control over the military. We still don't understand political parties very well. This is truly an important topic, which we have neglected. We don't understand why parties come into existence, what mechanisms hold them together, and what the glue of party discipline is. Though we have learned a lot in general about authori-

tarianism, I also think we know disastrously little about the structure of dictatorships. We tend to think that formal institutions are just a window dressing and yet my student, Jennifer Gandhi, found that they matter for the survival of dictators, for policies they pursue, and for the outcomes they generate. I think we are not doing well with globalization. I've written something on it recently, so I was forced to read the literature. I found it

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deeply unsatisfactory. In particular, the political consequences of globalization are poorly understood. I think the problem, in part, is that we need some kind of methodological breakthrough in this area of research. The methods that are currently used just don't do well enough. The findings are disparate, and most of them are based on statistical methods that assume that observations of particular countries are independent. So it is hard to believe the statistical findings. This is a big, important topic. Somehow we are going to have to start thinking differently and pay more attention to the sort of methods that would be appropriate for studying this issue. Generally, to a large extent because of the availability of data, we know more about the OECD countries than about the less developed ones. But this gap is rapidly closing.

**Q: You have emphasized the methodological difficulties of addressing complex questions in a**

***rigorous fashion. Another reason why progress on such questions might not be made is that comparativists simply fail to pose big, interesting questions about politics in the first place.***

A: What is it that we are not asking? Certainly, we are not asking “So what does all that we do know add up to?” But we also fail to ask several questions that are researchable with the methods we have. What determines the access of moneyed interests to politics? What is it about our democratic institutions that makes people feel politically ineffective? Why is it that these institutions perpetuate misery and inequality?

There is a saying in my native language, “It is not the time to cry over roses when forests are burning.” And as I talk to people in Argentina, France, Poland, or the United States, I hear that they are burning. People around the world are deeply dissatisfied with the functioning of democratic institutions, in the more as well as in the less developed countries. They see politicians as serving the interests of the rich, of multinational corporations. They cannot understand why democratic institutions seem to be impotent in reducing glaring and persistent inequalities. They feel that political parties do not serve as a mechanism of transmission of their values and interests. They perceive that important decisions are made by institutions, often international, over which no one has control. The danger is that unless we keep asking such questions, we leave the answers to demagogues of different ideological stripes.

The entire structure of incentives of academia in the United States works against taking big intellectual and

political risks. Graduate students and assistant professors learn to package their intellectual ambitions into articles publishable by a few journals and to shy away from anything that might look like a political stance. This professionalism does advance knowledge of narrowly formulated questions, but we do not have forums for spreading our knowledge outside academia; indeed, we do not commu-

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nicate about politics even among ourselves. It has been decades since professional journals - “professional” is what they are called - published essays on “What is wrong today with the United States, with democracy, or what not?” or on “How to make the world better?” We have the tools and we know some things, but we do not speak about politics to people outside academia.

Note:

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