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Letter from the Outgoing President: Introducing Comparative Politics

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The most compelling way to introduce comparative politics, or indeed any subject, is to do it, and to induce students to do it as well. What it means to "do" comparative politics is, however, up for grabs. This column will provide a model for one approach, which I have used in both a freshman course of 300 students and for an introductory graduate lecture class as well. My goal is to bring introductory courses more in line with theory-driven disciplinary practice.

The focus of an introductory course in comparative politics should be on outcomes (i.e. dependent variables) that differ across countries. These differences must be of some consequence for the students — that is to say, they ought to care deeply, or be induced to care deeply, about the value of their country, or any country, on the course's dependent variables. Democracy and its alternatives is clearly a candidate for selection, and I have used this as one dependent variable in all iterations of my course, from a total of three. Other dependent variables which I have used include: early economic development, late development, and lack of industrial take-off; experience or lack of experience of a social revolution in the country's past; the establishment of cultural homogeneity or the continued existence of cultural heterogeneity; openness vs. closedness to international trade and capital; and degrees of economic, social and political equality.

My opening lecture explains the "game" we comparativists play. To illustrate this game, I distribute a short passage from Montesquieu, trying to link egregious climates (the independent variable) to the popular rejection of tyranny (a point on the dependent variable) in England. I explain what a variable is, how, through the specification of mechanisms, we link independent and dependent variables, and the challenge of undermining theories by thinking up counter-cases (e.g. Russia in Montesquieu's time having the same value on the independent variable but quite a different one on the dependent variable).

The next set of lectures goes through the three dependent variables that will be the focus of the course. For each dependent variable I first go through the political theory canon to show the deep roots of the issue. I then go through contemporary analysis and examples to show why the value on that variable is important for people living in a particular country. Third, I show how the dependent variable can be dimensionalized (or made dichotomous), and how it is then possible to develop coding mechanisms to "fit" a particular country on the proposed dimension.

Finally, I go through the standard political science theories that purport to explain why countries wind up on different points on the dimension.

On democracy, for example, I start by outlining the theoretical formulations going back to Pericles. I then discuss the literatures on freedom, personal fulfillment, social mobility, the likelihood of war, and on economic efficiency that are all affected depending on whether the country is democratic. This supposedly demonstrates why the outcome is important. Third, I construct a dimension of democracy. Relying on Schmitter, I code countries based on the degree to which societal actors are free through elections and interest group activity, to influence government outcomes. The dimension goes from pluralism, to societal corporatism, to state corporatism, and finally to monism. Last,

Letter from the Incoming President: Comparative Politics/American Politics Conflict and Cooperation

Robert Bates
Harvard University

When I started out in political science in the late 1960s, comparative politics was marginal to the broader discipline. The sense of marginality was heightened by my location at Caltech, where the social sciences were marginal to the Institute; political science marginal within the social sciences; and the study of American politics king.

Caltech merely magnified patterns that prevailed elsewhere. In previous decades, comparative politics had shared in the launching of the behavioral revolution; the birth of political development as a subfield, for example, helped to consolidate the place of political psychology and anthropology in the broader discipline. But when rational choice theory displaced the behavioral revolution, students of American politics no longer took instruction from students of comparative politics. Relations between the subfields largely came to an end.

The politics of the 1960s helped to spark both transformations, that standing psychological commitments and the choices of voters could no longer be believed in an era whose politics was marked by the political emergence of such figures as George Wallace, Gene McCarthy, Barry Goldwater or Martin Luther King. From the recognition of issue voting, it was but a small step to the theory of rational decision making in the study of American politics. The turmoil of the 1960s affected the comparative field as well, but in a strikingly different manner. Students of development became students of Marxism, as the prevalence of revolutionary violence in the agrarian periphery seemed to confirm the arguments of dependency theorists.

Laitin, continued

I discuss three theoretical traditions that seek to account for this variation: Moore's focus on class alliances; Gerschenkron's on timing of industrialization; and Bendix's on the premodern structures of authority. It is here essential to calibrate the points of the dependent variable to be used in the lectures with the (quite often distinct) names given those points in the principal readings. For example, but only imprecisely, Moore's democracy includes Schmitter's pluralism and societal corporatism; Moore's fascism is Schmitter's state corporatism; and Moore's communism is Schmitter's monism. I follow the same exercise (generally limiting myself to three "big" theories culled from the literature on each of the dependent variables) for the other two dependent variables. The elaboration of each of these three dependent variables consumes no more than two lectures.

The course then moves from the theoretical to the empirical. I have generally chosen five countries, trying to build in as much variation on the dependent variables as possible. Also, because I have always assigned Moore's *Social Origins of Democracy and Dictatorship*, I have been somewhat guided by his country selections. My cases are usually Britain, France, Japan, the Soviet Union (I have not taught the course since 1991), and India.

Each country gets four lectures. The first lecture provides a political history of the country, and a preliminary coding of the three dependent variables of the course. Each of the subsequent three lectures focuses on a single dependent variable. These lectures open with an historical overview, with the goal of seeing whether the value of the dependent variable has been stable over time or changing, and as to whether we need a single account of the country's placement on the dependent variable or one that must account for variation over time. These lectures then go through each of the theories, asking whether each theory can easily account for the outcome, or whether amendments to the theory (e.g. some *ad hoc* intervening variables) are necessary. I emphasize during my lectures on Britain (the first country we examine) that any theory can account for a single case, even if it is somewhat anomalous. The "success" of a theory, I often repeat, is if it can explain the different outcomes over time in one country, and the different outcomes for a large number of countries, with only a minimum of *ad hoc* amendments. As we "move" from Britain to India, we get a clearer sense of what a robust theory must achieve.

Students write three papers, either comparing theories for a particular case or examining a theory over more than one case. I have found that college freshmen are utterly lost in writing their first paper, but by the third paper understand precisely what it means to have a theory "explaining" a case. In fact, by the end of the course, they can use evidence from the readings to challenge my judgments as to the country's placement on the dependent variable, to construct new intervening variables that apply to all cases, and thereby rid them of their *ad hoc* quality. They can, in other words, "do" comparative politics.

My final lecture sums up how well the theories have done to account for the range of outcomes that we have observed on each of the dependent variables and for each country. As an agenda for future research, it also points out apparently anomalous cases (among countries we have not studied) for which "successful" theories would need to account. I try to show that doing comparative politics, in constantly exposing theories to hard evidence, is like Sisyphus pushing a rock up a hill. The weight of anomalous evidence will tumble, over time, even the best theories. We can only begin anew. Despite this depressing prospect, I take sides with Camus when he demands that we must consider Sisyphus happy.

Bates, continued

Marxism failed, both politically and intellectually. Rational choice theory did not. Why, then, is so much of the present intellectual excitement in the discipline originating in comparative politics? Why have the subfields re-engaged in intellectual discourse?

Forces within American politics help to account for this renaissance. Nowhere were the excesses of area studies more obvious than among those, the Americanists, who most derided them! It became obvious to many that political scientists could not claim generality for theories that were tested only against a single case — and that the very case from which they were first constructed! The Americanists may have been arrogant, but they were also social scientists. Without admitting the validity of the comparativist critique, in search of variation, they have turned outward and joined in the search for comparative data. They have initiated the process of re-engagement.

Political events within the nations studied by comparative politics also contributed to the lowering of intellectual barriers. Most important was the spread of democracy. Students of Eastern Europe and the former Soviet Union, for example, found themselves desperately in need of the theory and methods that had long been standard fare for Americanists. Forces on the demand side thus joined forces of supply in directing attention abroad.

The vitality emanating from comparative politics possesses other sources. One is the concern with political economy; the other, a concern with cultural politics. Not only did democracy spread to other nations but so too did market reforms. The construction of capitalist systems highlighted the political foundations of the private economy, giving new vitality to the field of political economy and posing intellectual challenges. While theories of market failure provided a theory of the state's entry into economic life, the 1990s saw instead states creating markets.

The intellectual challenges posed by ethnic violence and religious fundamentalism also posed challenges to understanding. These challenges emerged even more sharply, given the spreading acceptance of rational choice theory within the subfield.

How should we in comparative politics respond to this unaccustomed centrality? I have several ideas, but will stress only one. It is to make common cause with Americanists in the training of graduate students.

Americanists now look to the world abroad as they rarely have previously done. And our students in comparative politics need to understand the work of Americanists. For our part, we should acknowledge, freely and with gratitude, that no political institution is as deeply understood as the United States Congress, for example. But, to paraphrase a colleague, we should remove the proper names off the study of it.

We should also recognize that students of American politics have long studied the behavior of *politicians*. Those of us in the development field have studied everything else but: corporations and classes, priests and shamans, students and intellectuals. We often look more like economists, sociologists, and anthropologists than like students of politics. By approaching the politics of the developing areas the way in which Americanists study the politics of the United States, we could, in all frankness, more directly focus on politics.

For our part we could, and should, not so gently remind Americanists that there are over 100 nation-states in the world today. Then we can, as political scientists, begin to see the general approaches and lessons embedded in their works. Our arguments can then be cast within a comparative setting, where they can be tested, rejected, or, refined. In dialogue with our colleagues, we can begin to do what we have always aspired to do: by engaging in comparative work, begin to build a social science.

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NEW OFFICERS

At its business meeting during the APSA convention in August-September 1995, the Comparative Politics Organized Section will elect a new president-elect, new secretary-treasurer and two new at-large members of the Executive Committee. The nominating committee, made up of Barbara Geddes (UCLA), John Curtice (University of Strathclyde), Sam Nolutshungu (University of Rochester), and Richard Samuels (MIT), proposes the following slate:

President-elect: David Collier, University of California, Berkeley
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Three Italian Responses to Robert Putnam's *Making Democracy Work*

Robert Putnam's prize-winning *Making Democracy Work: Civic Traditions in Modern Italy* (Princeton: Princeton University Press, 1993) has been hailed as a major achievement in the comparative study of politics. The *APSA-CP Newsletter* offers its readers three critical reviews by Italian scholars which, because they were originally published in Italian, might otherwise have escaped notice in the U.S. All translations are by Francesca Godi.

Robert Putnam respectfully declined to respond to these three reviews presented in this *Newsletter*, choosing instead to focus on his current and different line of research. He directs readers to a review of *Making Democracy Work* by Sidney Tarrow in an upcoming issue of the *APSR*. In that same issue Putnam responds to what he sees as two broad misunderstandings of his research. The first misperception is that his work is a theory of political culture and the second is that it is historical determinism. Both of these points, Putnam relates, lead directly into his current and future work.

Regions, Civic Tradition and Italian Modernization

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University of Turin

The seriousness with which Putnam's ten year long study was formulated and carried out goes without saying. It is also true, though, that during the course of the work the scope of the analysis was broadened; the answers to the initial questions on the performance of regional institutions led to other questions that moved onto less solid terrain. The finding of long-term historical effects and their reinterpretation is uncertain, and even if Putnam claims that his larger interpretation is only a stimulus to further reflection, the book nevertheless leaves us open to possible errors.

I assume the reader has sufficiently detailed knowledge of the work (and of Italy). I will not, therefore, summarize the study, but will instead limit myself to some observations on the long-term consequences of the differences in the origins of Italy's regions. The issue that I address has to do with the insistence on "civicness" as an explanatory variable not only of regional performance, but in effect of the general state of contemporary regional societies.

No one doubts that institutional traditions are important. Likewise, referring to partial and micro mechanisms is appropriate to understanding the functioning and transformation of societies. This highlights an important point regarding the nature of sociological explanation.

A long-term historical comparison, like the dizzying comparison of structural analogies of different societies separated by a span of many centuries—but even those separated by merely a hundred years—in effect compares two societies in their entirety. The comparison of a single element extrapolated from the rest has only limited meaning.

Let us take the case of the region of Emilia-Romagna, considered by Putnam the region of civicness *par excellence.* Without addressing the issue of whether it is the tradition of civicness that explains contemporary regional efficiency, is it the civicness of the past that explains economic development today? To explain the achievements of this region—and similarly of other regions whose economies center on small firms—it is not enough to isolate a single factor. As many studies have shown, it involves the very fabric of medium-sized cities, of the independent productive relations in the countryside, of distinctive local politics, of a particular family structure, and it involves models of production that even 100 years ago were already embryonically those that at the time of industrial transformation would unexpectedly become efficient again, as well as various other things.

A complex system of variables internal and external to the region can explain its economic success, to which the tradition of civicness has certainly *also* contributed a great deal. But it is the combination of these elements that is decisive if we consider the long-term, and it is difficult to say how much impact civicness has had or how much impact other structural factors have had.

A concise indicator, such as a low level of industrial employment at the beginning of the century, is not enough to delimit the importance of specific pre-existing economic structures for subsequent development. In the countryside of the Third Italy [as the central regions dominated by small-scale industry are known; Editor's Note]—in some areas, not in all—women from agricultural families who worked braiding straw in their homes, for example, and merchants who traveled the world selling hats (neither of whom were usually included in the census counts at the beginning of the century) had already prepared the way for an organizational model that would become the basis of the diffused postwar economy. These factors at the origins of the economy, factors extremely difficult to measure, and not those having to do with the presence of large industry at the beginning of the century, are important in predicting subsequent growth. In fact, there is no correlation between large industrial works then and small firms today in those regions. The model that eventually developed was latent, and might have disappeared altogether if other technological and market conditions appropriate for its selective adaptation had been absent. When it was reactivated, it consisted of a specific model for which community civicness was of great importance.

Finding a structural comparison between two distant societies does not serve to "explain" continuity. What is "in the middle" is certainly more important than the big continuities. Likewise, even the vertical structure that exists today in Southern Italy is something very different from the vertical structure that existed several centuries ago. It developed in unpredictable ways, as an outcome of actions and interactions which no one at that time could have predicted. It was not within that structure any more than a statue is in a piece of marble.

Putnam can in reality claim to be in agreement with all we have said. He would, however, defend his argument about the long-term saying that it is meant to provoke reflection, even though he is convinced of the complexity of historical change. In a moment, though, we will show that one can assemble, for example, another very different interpretation of Italian historical continuities, one which also has a certain plausibility. This reasoning leads us to question the author about the logic of an explanation relying on mechanisms; a logic that he too shares.

It is in making evident and understanding the interaction of a quantity of mechanisms, as many as possible, activated as expected or unexpected consequences of social actors, that we understand a process of historical change or continuity (Elster, 1989, but also Boudon, 1984 or Friedberg, 1993). It is understanding the outcome of these interactive games that in the end allows us to decide whether continuity has occurred or not, because no one general theory of change is satisfactory and no single long-term comparison sufficient.

For clarification, I will give some recent examples of mechanisms which supplement some of those indicated by the author, and which complicate the simple civicness thesis. I refer to three:

(i) The importance of the artisan industry for economic growth and for the production and reproduction of networks of horizontal relations has been demonstrated. In the postwar period, with the most aggressive small industries of the North-Central region gaining access to southern markets, the important pre-existing fabric of the southern artisan industry was wholly leveled. This mechanism shows that an economic condition for the development of civicness, while it may have been decisive elsewhere, was destroyed in the South by external economic activity. No one can tell how much this may have hindered the formation of a "new" civicness at a crucial moment.

(ii) The province of Reggio Emilia can be considered the heart of civicness. Networks of associations and Chambers of Labor are well known here. That politics in this area has taken this road, and not that of the more radical and often zero-sum organizations and conflicts of other areas that are also characterized by agricultural laborers, has to do with the fact that in Reggio Emilia at the beginning of the century there existed one of the most complex class structures then known to the pre-industrial world, one made up in equal shares of laborers, small business owners, sharecroppers, and tenant farmers. Under these conditions, the organization and the mediation of interests found what we might call its "natural" form in the "Chamber of Labor," an institution of civicness *par excellence*. What was most important to this outcome, class structure or pre-existing civicness?

(iii) A few years before new market forms and new technologies made the model of development based on small-scale industry possible, after the long wave of Taylorism, observers of the Veneto region came to the conclusion that there was a tendency towards the "southernization" of the region, that is to say, toward the formation of an economy characterized by public intervention and political nepotism. What would have come of the civicness of the Veneto region if an unpredictable and exogenous pivotal economic change had not made possible the selection from cultural heritage of a social model appropriate for the new and unexpected return to the scene of small businesses based on high technologies?

Understanding many mechanisms such as these—that are not evident in correlations of aggregate data—can direct our historical interpretation of continuity. These mechanisms cannot be deduced from any concise image of the past.

The final part of Putnam's book is dedicated to carrying forward his results in the context of the theory of social capital (Coleman, 1990). Putnam did well to place himself in that current, and with the persistent rhetoric of civicness, he wanted, in the end, to convince us of its importance. Of this we were and are still convinced, even if in perhaps a more moderate sense. All that remains is that the book is *not* an explanation of Southern backwardness. It risks being misunderstood, and it tells us little of what we should do until we have freed ourselves from the exaggerated analytic weight of long-term thinking, multiplying instead our intimate knowledge of social mechanisms in many directions.

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*A longer version of this review was originally published in *Stato e Mercato*, no. 40 (1994).

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Coleman, J.S. (1990), *Foundations of Social Theory*, Cambridge, MA, Harvard University Press.

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Paths of Development

Antonio Mutti*
University of Pavia

Putnam's work is presented to the reader as the mature fruit of 20 years of research in the field and of serious studies of Italian history and society. He commits himself to identifying an empirically controllable explanation of the causes that produce differences in the performance between the regions of the Center-North and the South, *as well as those that exist between the regions inside these two large areas.* Drawing upon insights that come from the current theoretical framework known as "neo-institutionalism," Putnam affirms that institutions should be viewed in their social environment and that there exists a reciprocal influence between institutions and environment. From the first page, it is clear to the reader that Putnam's main interest is to explain the environment's influence on institutions.

By concentrating his research on an effective explanation of North-South dualism, Putnam is forced to minimize, or worse yet, to pass over in silence, the differences inside these two macro areas. One could say this is an inevitable limitation since, if one wishes to identify common features, one is forced to abstract from the differences. The question is whether such a limitation carries with it an acceptable or excessive cost, especially with regard to suggestions, such as those formulated by Putnam, for public policy as regards development. It then seems to me that Putnam pays an excessive price, sometimes even forcing the evidence provided in the data he submits to the readers' attention.

If one observes the central figure in the work (fig. 4.5), one discovers that there certainly exists a strong positive correlation between the level of civicness and the level of institutional performance considering the Italian regions in their entirety, and that the figure split into two quadrants, with the Center-North regions in the upper and the regions of the South in the lower. Much less strong, even though acceptable and statistically significant, are the correlation coefficients that, inside each of the two quadrants, tie the performance of regional institutions to the level of civicness. A marked dispersion of values is strongly apparent, especially in the quadrant of the southern regions. This produces more than minor distortions about which Putnam, not coincidentally, remains silent, content with the fact that the correlation works. These distortions should instead be more attentively statistically assessed and treated. Otherwise, and in spite of the validity of the correlation, they end up raising the following questions, the answers to which are not found in the text and which inevitably bring other variables into the picture, complicating the interpretation furnished by the author. If there is a strong positive correlation between a sense of civicness and the performance of regional governments (and communal governments, according to appendix E), how is it that Basilicata, with a level of civicness lower than that of the Abruzzo, has the same level of institutional performance as the latter? Also, how is it that Sardinia, with a level of civicness slightly higher than that of the Abruzzo and much higher than that of Basilicata, reports lower levels of institutional performance with respect to both these regions? Analogous questions can be posed comparing Puglia and Molise with Sardinia, and even some regions of the Center-North among themselves.

There is no answer to a question, why is it that Molise, Basilicata, Sardinia, and Sicily, which had lower levels of civicness in the period 1860-1920 compared to Campania and Calabria, exceed these regions in the 1970s both in the level of civicness and in terms of institutional performance? It is then natural to point out that traditions of civicness are not stationary over time, as Putnam affirms, but evolve even inside the South.

What strikes the sociologist most is the uncritical use of categories like familialism and clientelism which, precisely because of the strong explanatory power attributed to them, merit a more careful assessment, especially on the part of those who have been so interested in operationalizing concepts.

I am not arguing that the thesis of the South's family and client-based particularism with respect to the rest of Italy is wrong or unproductive. Rather, I argue that, in order to be used coherently, even in order to identify an effective and differentiated development policy, this approach would have to provide some answers to at least these two questions.

(i) How does one understand, not only quantitatively but also qualitatively, the phenomenon of particularism, and also the lack of cooperative spirit, generalized trust, and horizontal solidarities in various parts of the South?

(ii) Do forms of particularism exist that could adapt *because of their own characteristics*, and not solely because of environmental factors, towards more "modern" solutions?

These two questions may seen banal because, in the end, they correspond to a plea to improve study of the socio-cultural and political dimensions of the different parts of the South. But, in reality, this is not at all the case if one thinks that the lack of this type of research is truly startling. All this, I continue to believe, is not only the product of the backwardness of social research in the South, but also of the conventional intellectual formulations that end up blocking this kind of research from the outset, producing a self-limiting vicious cycle.

The question that these approaches carefully avoid posing can be formulated as follows: do configurations of particularistic relationships (family or client-based) exist that may be more suitable than others to not only coexist with development, but also make a positive and autonomous contribution to modernization?

Several studies offer, more than do others, a basis of useful work from which to start. I am thinking, for example, of Dellile's research, which identifies the following fundamental division in the South: on one hand, the rural areas of small and medium sized plots characterized by specialized cultivation and by the diffusion of rural artisans; on the other hand, the areas of coastal or interior plains dominated by large agricultural estates, by extensive agriculture and by the existence of a large rural proletariat that is poor and mobile. While in these second areas property is passed on bilaterally and is uxorial, in the first areas transmission is centered on male lineage, patrilinealism, and the presence of large familial groups. These are what consolidate the ties between family and large scale farms, thereby reinforcing the economic role of the family. This is a matter of a preliminary and interesting attempt to link family structures, networks of relatives, and entrepreneurial strategies.

The confines of the network of relatives are specified. To which levels of the network of relatives do exchanges, solidarity and reciprocal help extend? It is clear in fact that the network of relatives can act as an important resource in economic activity, but it can also present an excessive cost when the expectations of assistance from a member in failing economic conditions exceed certain thresholds. The ability to fix virtuous confines, not too wide-ranging and not too limited, to the support provided by relatives to the entrepreneurial activity of its own members constitutes, therefore, an important variable. According to certain authors, this should be precisely at the base of entrepreneurial success of certain ethnic and religious communities.

Finally, it is necessary to specify the level of openness of family to people outside the family and the network of relatives (whether friendship, neighborhood, or other ties). The more open people are and prepared to build broader ties with other institutional groups and spheres even in terms of economic activity, the more they reveal themselves to be suitable to the modernization process.

With respect to the client relationship, in the version of clientelism involving notables as well as in the more modern version of political party clientelism, we have without a doubt some valuable research, although by now a bit dated. These have shown us the negative effects of this type of social and political integration for the level of equality, social justice, effectiveness and efficiency of public action. The generalization that emerges with time from this research, though not sufficiently demonstrated empirically, is that the client relationship has always and everywhere in the South assumed the same forms and content, and that it has always and everywhere proven to be a constraint on modernization. From this comes the inevitable conclusion of the total incompatibility of this relationship with development policies precisely because of its internal structure. The configuration of trust underlying the client relationship, this perspective maintains, never appears connected to technical competence and responsibility as may have happened in other parts of the country or as appears evident, for example, in the case of Japan.

I think that this generalization has not been completely demonstrated empirically and that it has ended up often becoming a simple and overused trigger for political battles. One could give several examples of local development in the South where clientelism associated itself, as in other situations of the Center-North, with development policies. I limit myself to highlighting the most macroscopic case. The experience of development in Abruzzo, a region that in the early 1950s was certainly not classifiable among the more favored in the South, has until now aroused only the interest of economists who are always more attentive than sociologists and political scientists to the analysis of the internal differentiation in the South. Abruzzo also constitutes, though, an interesting case of social and political development that merits more attention on the part of other scholars. The classic indicators of clientelism (the preference vote, disability pensions, etc.) place Abruzzo, as Putnam himself shows, in an intermediate position with respect to other regions of the South. Notwithstanding the level of clientelism which cannot be neglected, institutional performance in this region seems reasonable, even according to Putnam, together with that of Basilicata, the highest among the regions of the South. Such performance cannot be explained on the basis of the existence of an elevated level of civicness. The very data provided by Putnam suggest this, as we have highlighted. In fact, if further explanatory variables are not introduced one cannot understand, for example, why Sardinia reports institutional performance decidedly lower than in Abruzzo although the former has a level of civicness slightly above that of Abruzzo. I believe that a significant intervening variable is the local and regional political system; more precisely, the way in which power relationships have flourished in the postwar period within the political elite.

In fact, we find ourselves facing a stable and cohesive political elite in spite of the strong parochialism that marks the social fabric of the Abruzzo. Such cohesion is favored by the presence, in the last 20 years, of monolithic leadership (Remo Gaspari) that has functioned to centralize local and regional politico-institutional power. Such leadership has undoubtedly managed consent through the classic client-based didactic-distributive mechanism, but has also created a second, more regulatory, client-based circuit directed to different areas and different regional sectors. This second circuit has been held together not only by the centralization of political power but also by a minimal idea of diffuse development that has satisfied various localities by favoring industrialization without paying costs that are too high as regards coherence and functionality of the actions undertaken.

Providing this example was not intended, obviously, to propose such an experience as a model of political development for the South, but only to highlight that it is very likely that the client-based political systems of the South, each in its own way, have provided differentiated levels of support for development. All this requires an approach to the study of local and regional political power that goes beyond the important interpretative models

The Politics of Civic Tradition Eclipsed

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University of Bologna

Which factors explain why some Italian regions are governed more effectively, much more effectively, than others? After a fascinating research trajectory, Robert Putnam ends up endorsing and trying to prove that one explanation is clearly more plausible than others. Better still, that only one convincing explanation exists. It is not the level of economic development that explains institutional performance, as much of the literature on democracy maintains. The author maintains that it is not even either the formalities of government or the party composition of the regional government (the point is never explicitly or deeply confronted).

In fact, Putnam himself realizes he is forcing his interpretation. At various points in his research he seems to have felt the need to introduce explicit cautionary notes for the reader, notes that place his explanation in a new perspective. For example, "civic traditions alone did not trigger (nor, in that sense, 'cause') the North's rapid and sustained economic progress over the last century" (p. 159), and "it would be ridiculous to suppose that the civic traditions...are the only—or even the most important—determinant of economic prosperity" (p. 161). In spite of this, Putnam never poses to himself the problem of the effect of government and policy on economic development and institutional performance with clarity and precision. Rather, when he does something like this, he resolves the problem by appealing to the iron law of civil life expressed by Machiavelli: "That it is very easy to manage Things in a State in which the Masses are not Corrupt; and that, where Equality exists, it is impossible to set up a Principality, and, where it does not exist, impossible to set up a Republic" (p.132).

If I have understood them correctly, the civic communities that Putnam identifies in the Center-North and South have arisen and maintained themselves despite dramatic political, economic, social and religious changes, and have reinforced themselves in the course of 7 or 8 centuries of separate development. The initial social capital—reciprocal trust plus the willingness to collaborate—accumulated in the regions of the Center-North in the period 1100-1300 allowed them to become and remain characterized by a sense of civicness despite all subsequent turbulence, while the regions of the South, denied this social capital because of the political organization introduced by the Normans, have never again been able to acquire it. Therefore, at least at the beginning of this long, exhausting, and troubling multi-century process we can see that it was the type of political organization of the Normans in the South and of the town councils in the Center-North that established the bases that rapidly became substantially decisive. Also, in the first case the political organization did not allow, and in the second facilitated the initial investments of reciprocal trust, solidarity, and mutual aid, indispensable elements for building the social capital of civic community. At least in that period (but why only in that period?) the political organization and the mode of government can be considered determinants. At least this must be true according to the interpretation provided by Putnam himself. Following this the author writes of the necessity of identifying "social equilibria" rather than causes and effects. I interpret this methodological note as a suggestion not to think that a cause and effect relationship exists between a certain type of political organization and specific civic traditions. Problematically, I suspend judgment.

According to Putnam, there are two possible social equilibria. The first is based on the reciprocity/trust duality; the second on the dependence/exploitation duality. "Reciprocity/trust and dependence/exploitation can each hold society together, though at quite different levels of efficiency and institutional performance" (p.178). "History determines which of these two stable outcomes characterizes any given society" (p.179). Here lies my major point of dissent. I would, obviously, be tempted to write: it is politics that decides which of these two stable results (but how stable, and for how long?) characterize a given society. That is to say that I would look to conflicts and struggles, the forms of the organization of political life, and the governmental institutions of the political community to find the roots of the dualities reciprocity/trust and dependence/exploitation. I would want to know, for example, if, even when the first duality appears, the penalties against deviants that Putnam identifies as "transgressors and loafers," that is, against those who do not operate on the basis of reciprocity and trust, are not and cannot be solely social. There must be, particularly and in a special way, in the present and in the foreseeable future, political and institutional sanctions. In essence, a sense of civicness is created and maintained when citizens know that close-at-hand there exists a level of government that will protect them, right wrongs, bring about justice, bestow punishments and hand out compensation or direct individual objectives in the direction of attaining equally satisfying collective objectives.

Two problems arise in this regard which Putnam does not deal with directly. The first is in what way and how long good institutional performance produces social capital. The situation would seem a hopeless one. If a region does not have a sense of civicness, then it will not have regional governments

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Mutti, continued

provided by Putnam.

*A longer version of this review was originally published in the *Rassegna Italiana di Sociologia*, vol. 35, no. 1 (Jan.-March 1994). Page references to the Italian version of *Making Democracy Work* are omitted. For a more detailed presentation of the author's argument, see Antonio Mutti, "Il particolarismo come risorsa: politica ed economia nello sviluppo abruzzese," *Rassegna Italiana di Sociologia*, vol. 35, no. 4 (Dec. 1994): 451-518.

Economies of Scale and NAFTA

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Recently, the world has experienced a spate of trade pacts signed by regional trading partners in an effort to lower tariff barriers. If free trade is beneficial, why do nations settle for regional pacts, rather than pursue universal free trade? Helen Milner (Columbia University) uses this question in "Industries, Governments and the Creation of Regional Trade Blocs" (unpublished paper, March 1995) as the springboard from which to argue that

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Pasquino, continued

capable of good performance and consequently, it will practically never be able to accumulate sufficient social capital. This seems to me a truly vicious circle. It is interesting that Putnam indicates his skepticism in the matter with two revealing observations. The first is: "Only when the PCI (now rebaptized the 'Democratic Party of the Left') gains power in adverse circumstances of that sort will it be possible finally to evaluate the claim that party control makes a difference for good government" (p.119).

The second observation is along the same lines. It also serves to reassure all those who criticized him because, in short, the red [i.e. communist-run; Editor's Note] regions, Emilia-Romagna *in primis*, always and inevitably appear at the head of all his measures of institutional performance. This is true, Putnam responds, but it does not depend on the political composition of government: "The more civic a region, the more effective its government" (p. 98). It may be that the accumulation of social capital is as difficult and long a process as it is unforeseeable and unlikely. Nevertheless, one could at least give credit to some regions, and to their political ruling classes, for not having wasted the social capital accumulated by their history and for having known how to exploit it. Therefore, at least in these cases—and again the success story is that of Emilia-Romagna—one must attribute some weight to politics, some role to government for the investment process and the exploitation of social capital.

Putnam genuinely has some doubts on the matter. Curiously, he writes that the good performance of some regions could in effect be attributed to the PCI. But this would be a result of "a rational, competitive calculation on the part of the PCI that it could best establish its credentials as a national party of government by showing how well it could rule regionally and locally" (p. 119). In short, the communists governed more effectively than others not because they were more competent, more honest, organizationally stronger, or more capable of satisfying collective preferences by offering collective goods, but because of "a rational calculation". Yet, a doubt arises even in Putnam who, even in turning to the cosmetics of technical terms, finds himself compelled to admit that "by the time of our later, fuller evaluation of institutional performance, the correlation between PCI power and institutional performance was not entirely attributable to covariance with the civic community" (p.119). In other words, and perhaps more clearly expressed, it could be that the communists, their political organization and their mode of government made a difference over and above that of the civic tradition of the regions which they governed—and the traditions of which they had in some way powerfully contributed to building. Therefore, there really exists a space for politics and a role for government that can spur higher, better levels of institutional performance even in communities that do not have a particularly substantial or exceptional sense of civicness.

I think that Putnam would say that governments can do only what the civic traditions of their context and of their citizens' consent permit them to do. The limits regarding revolutions, "confining conditions," as Otto Kirchheimer called them, seem like strict ones, almost predetermined, and not easily crossed. For my part, I support, in fact, the relative autonomy of politics. Finally, it is precisely this, politics, the element that seems to me to be glaringly absent from Putnam's analysis. Given and accepting the fact that civic traditions matter and that Putnam not only does well to highlight this element but does just as well in researching the historical construction of these civic traditions, what space opens up for politics? That is, what practical role is played by those men and women who organize, struggle, suffer, and accept deferring their personal goals, which at times they never obtain, in order to work on collective goals? What role is played by those rulers who exploit the existence of civic traditions to spur the institutional performance of their regions to the highest levels?

Civic traditions matter, but civic behavior and the behavior of government matter even more so. If it is true that "changing formal institutions can change political practice" (p.184), then not only "social context and history profoundly condition the effectiveness of institutions" (p.182). It is also, I continue to believe (perhaps above all), what political organizations, their managers, and their representatives in the bodies of government do and don't do that significantly affects the conservation and transformation of civic traditions; and it is perfectly right that this should be the case.

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*A longer version of this review was originally published in *Polis*, vol. 8, no. 2 (Aug. 1994). References are to the English-language edition of *Making Democracy Work*.

trade pacts reflect national governments' reactions to the pressures of private economic agents. In contrast to others who argue that international pressures have caused the recent flurry of regional trade agreements, she focuses on the interests of domestic industries to explain the occurrence and the content of the pacts. While this "second image reversed" method of explaining national trade policy is nothing new, Milner improves upon the existing literature by providing a careful theoretical explanation as well as a thorough test of her argument by using more comprehensive data. Moreover, she offers a new twist by arguing that economies of scale is the salient factor driving trade liberalization.

To support the claim that national policy reflects domestic interests, Milner provides a conscientious three-step argument to show how micro-level preferences become expressed in regional trade pacts. She first identifies industrial preferences. She argues that firms seek liberalized trade in order to exploit scale economies in a larger market. A firm has scale economies if its average cost decreases as it expands output; in other words, it produces with "increasing returns to scale." As trade barriers fall, the market size increases, enabling these firms to produce more with fewer costs. Not all industries have scale economies. Industries such as agriculture and petroleum possess little or no scale economies, while industries such as telecommunications and automobiles experience large economies of scale. Likewise, industries with higher economies of scale will prefer a larger market, and hence more liberalized trade; and industries with lower economies of scale will prefer a smaller market and barriers to trade.

The use of trade theory based on economies of scale contrasts with previous scholars who have relied on neo-classical trade theory. The latter, the Hecksher-Ohlin model of trade, assumes perfectly competitive markets and predicts that countries with different factor endowments will trade dissimilar goods with each other. However, most economists agree that markets today remain imperfect and that intra-industry trade, the exchange of similar goods, has become increasingly important in the postwar period as countries have modernized.

The next step in formalizing Milner's argument is to show how industrial preferences become translated into national policy. She first assumes that national leaders' interests are to stay in office and that re-election is based on the economic situation of the voters. However, leaders must balance their interest in maintaining tariff revenues with voters' interests in high consumer surplus and firms' interests in profits. The utility function of national leaders combines consumer surplus, firm profits and tariff revenues, to analyze whether leaders will chose between 1) a protected home market, where tariffs are high but consumer surplus is low; 2) multinational trade liberalization, where tariffs are minimal, consumer surplus is high, and firms' profits are low; and 3) a regional trade agreement, where firms' profits are highest. "Regionalism can represent a middle way, which sacrifices less in consumer surplus and tariff revenues than the other two options, while emphasizing firm profits" (p.19). Thus, regional trade pacts are more common than multilateral free trade.

The final step in Milner's argument is to explain why nations do not lower trade barriers unilaterally across industries. First, trade is driven by increasing returns to scale (IRS) industries. Milner writes, "Regionalism essentially involves trading scale economies with other states." Thus, whether or not a trading partner will agree to lower tariff barriers will depend somewhat on whether it also gains from "swapping" markets access. Increasing the number of IRS industries in each country increases the probability of an agreement. She also notes that trade liberalization enhances competition and that not all industries will increase production. Some will be squeezed out of the market by more competitive firms. If an industry has a large amount of differentiation, it implies that a country would be less likely to lose the whole industry after trade. Countries would not be trading whole industries, only parts of industries (p. 24).

The case which Milner uses to test her hypothesis is NAFTA, the free trade agreement between the US, Canada and Mexico. The assertion that intra-industry trade characterizes exchange between Mexico and the US and Canada is controversial. However, she cites evidence that in 1994 three of the five goods most traded between the US and Mexico were the same (p. 27). Hence, it appears that trade is driven by the pursuit of exploiting scale economies.

To test whether the level of scale economies can determine the level of tariff reduction, Milner analyzes seven US industries and the NAFTA terms which apply to each. These industries are telecommunications, automobiles, financial services, transport equipment, textiles and apparel, agricultural products and energy. She first ranks each industry based on their level scale economies, her key independent variable. Economies of scale are measured by the minimum efficiency scale (MEPS), an estimate of the minimum firm size needed to reach an optimal scale of production.

Though scale economies is Milner's most salient independent variable, she then regresses numerous other variables to explain the level of tariff reduction in the NAFTA accords. It is unclear why she includes in her analysis other variables, such as human capital intensity, export-orientation and industry growth when her argument is based on increasing returns to scale. In the end, the regression results show that these extraneous variables are not significant explanatory variables. Only the variable measuring scale economies is significantly and positively related to the level of tariff reduction in NAFTA. She concludes that US industries with scale economies received more trade liberalization in NAFTA than others.

Though Milner's results seem quite convincing, her argument could be strengthen by a cross-national test. Her argument is easily generalizable to other regional trade pacts. Indeed, she often mentions the European Union as a case that would most probably support her

The Imperatives of Regional Studies of Africa: Directions for New Research

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The ending of the Cold War has had a profound effect upon the international as well as domestic relations of countries throughout the world. Nowhere is this more true than in Sub-Saharan Africa. The superpowers, Russia and the United States, who had for almost four decades been committed to intense competition for ideological hegemony throughout the world, have now decided to instead cooperate. In the process, Africa is in grave danger of being marginalized or put up for *triage* in the world system. As in other regions, international relations in Africa is no longer mostly global in orientation, but regional. African states are experiencing domestic conflicts that often spill over borders, presenting problems for regional security. They are also faced with economies that are in a shambles, and crying out for regionally based recovery strategies. At the same time, the major powers of the world, as well as international organizations, are reluctant to intervene except for humanitarian purposes, encouraging instead that regional initiatives be taken to prevent and manage such problems.

Faced with the realities of the "New World Order," African leaders have decided to fight the trend toward the marginalization of the continent, and to attempt to find African solutions to African problems. The main problems confronting Africa today can be subsumed under the rubric of "security." However, instead of this term referring exclusively to politico-military situations, security is now viewed as a total generic concept that includes such dimensions as poverty, underdevelopment, migration and refugee flows, economic development, and inter-state as well as domestic conflicts. None of these problems are exclusive to any one country or any sets of countries. They are continent-wide problems that are most reasonably approached at the sub-regional level.

One of the by-products of the ending of the Cold War was a shift in the policies of bilateral and multilateral aid agencies who are now applying conditions to the aid they dispense to recipients in developing countries. Political liberalization and/or commitment to effective and efficient public management, or good governance, are now prerequisites for most foreign assistance. Poor governance and bad policies had created, over the first three decades of African independence, circumstances that by the end of the 1980s had become unbearable for the citizens of many African countries. This led to the emergence of popular movements for political and economic reform. In places where authoritarian regimes were most intransigent, such as Ethiopia, Sudan, Somalia, Rwanda, and Liberia, armed movements emerged and assumed military capacities that were unimaginable 15 years earlier. The Cold War had generated a trade in arms that laid the basis for opposition groups to match the force of established armies. In other places, economic reform and external pressure on authoritarian regimes to democratize served to catalyze an emboldened civil society that began to press for political liberalization.

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hypothesis. Nonetheless, her results would have been more powerful if she had compared her results for the US industries to the same industries in at least one other country, Canada or Mexico. A comparison to another democratic country would also add further support to her inferences about how domestic preferences are translated into national policy. Furthermore, a comparison between the US and Mexico, for instance, would show that industries with scale economies have similar views of trade, despite each country's different levels of industrialization and factor endowments.

Though the introduction to her paper poses the question, why are countries interested in trade agreements now, Milner cannot explain timing based on the argument about economies of scale. Industries that have scale economies today surely had them ten years ago. Though this criticism does not point to a flaw in her argumentation, it does show the importance of international variables to explain at least one aspect of trade agreements, timing.

The recent paper by Helen Milner gives a thoughtful analysis of national trade policy. She uses the literature on economies of scale to explain the variation of industrial interests, without ignoring the politics involved in national leaders' decisions to formulate trade policy. Though the latter link in the causal chain is more theoretical, her results support the domestic interests aspect of her story. Milner's argument is easily generalizable to many other cases of regional trading blocs. This paper is sure to shed new light on this topic for students of political economy.

For a copy of the paper discussed here please write to Helen Milner
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In Africa today, the trend toward political democracy has had the unexpected consequence of heightening ethnic tensions; and in some places such as Ethiopia, Liberia and Rwanda, this has resulted in domestic ethnic tensions that have on occasion spilled over into neighboring states.

One of the defining features of what is now commonly referred to as the "New World Order" is the emergence or resurgence of nationalism among large ethnic groups heretofore incorporated into multi-ethnic states. Not only is this an everyday fact of life in the former Soviet Union and the former Yugoslavia, but countries like Ethiopia, Somalia and Sudan have experienced similar fates. In other places such as Kenya, Zaire, and Mauritania, the intensity of ethnic nationalism may be lower, but nevertheless, it has the effect of creating a displacement of minorities under stress from other ethnic groups that want to purge them from regions they have inhabited for generations.

What is important about contemporary domestic and regional conflicts in Africa is that they now have the propensity to become internationalized. The current conflicts in the Horn of Africa and Rwanda, for example, have created refugee flows and the flow of armed combatants across national borders, catastrophic famine, and gross violations of human rights. In the process, what were once thought to be mere domestic conflicts, out of the purview of international organizations like the UN and regional organizations like the Organization of African Unity (OAU), have now been internationalized. The major world powers see the potential for humanitarian crises in Africa growing into regional political crises that could result in outsiders being drawn in for whatever reason unless regional conflict management mechanisms are put into place.

Consequently, the international state system is now being forced to rethink the notion of state sovereignty, and is being challenged to establish new rules to govern when and how international and regional organizations should intervene in domestic conflicts with international implications. In response to the new situation, the OAU has established a Conflict Resolution Mechanism. The primary objective of the Mechanism is said to be the anticipation and prevention of conflicts. In situations where conflicts have occurred, the Mechanism is supposed to be responsible for undertaking peace-building activities. This is an ambitious project that will be difficult to implement and expensive to maintain. Outside assistance will be required to sustain it. Although financial commitments have been made by the United Kingdom, US, Italy, Indonesia, and China, support from many more non-African countries will no doubt be needed to enhance the institutionalization of the Mechanism.

Given the changed political and economic circumstances on the ground in Africa today, students of the region must reconsider the paradigms and approaches they use to study the political economy of change on the continent. Although some tentative steps have been taken toward understanding contemporary issues of state sovereignty and regional security in Africa (see Edmond J. Keller and Donald Rothchild, eds. *Africa and the New International Order: Studies of State Sovereignty and Regional Security.* Boulder: Rienner, February 1995), the field is wide open for new research in Africa that approaches political economy and international relations questions from a regional perspective. Until recently, for example, the tendency had been to consider the proper focus for the study of international relations as global rather than regional. However, recent events have shown that the challenge of the 21st century will be to develop analytical approaches that allow us to bridge the gap between international relations and comparative political analysis, so that we might better understand how domestic ethnic and religious conflicts impact upon internal as well as sub-regional and regional relations. Such research would be of both discipline and policy relevance.

In addition to focusing on the origins, dynamics, and possible resolutions to ethnic and religious conflicts, comparativists should begin to critically examine efforts in Africa to develop mechanisms for regional and sub-regional cooperation not only in political matters but also in the realm of economics. As has been demonstrated by the Economic Community of West African States (ECOWAS), the Intergovernmental Authority on Drought and Desertification (East Africa), and the Southern Africa Development Community, what begin as organizations to promote regional economic cooperation often end up being much more, involving themselves in politics as well. The worldwide trend toward the establishment of regional common markets has also taken root in Africa. ECOWAS has been in place for more than a decade and the Preferential Trade Area in East and Southern Africa, for a decade and a half. Neither of these institutions, however, has been able to mature into a full-fledged customs union. In southern Africa, the recent revival of the Southern African Development Community, now including South Africa, could show the way for the development of a viable regional customs union. This was made possible as much by the ending of *apartheid* in South Africa as by the exigencies of the New World Order. We could learn much from the attempts to establish new sub-regional trade regimes and other forms of economic cooperation. Such developments will no doubt impact significantly on inter-state as well as intra-state relations.

In the near term, Africanists should initiate studies in the political economy of development and international relations from the perspective of regions and sub-regions. Necessarily, such studies will bridge the boundary between comparative politics and international relations, concentrating on the impacts of exogenous and endogenous factors on regional as well as domestic politics.

Comments on Replicability and the Study of Comparative Politics

Miriam Golden
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In the last issue of this Newsletter, David Laitin asked for reactions to a revised version of what I shall call the King replicability statement, after its author, Gary King (see *APSA-CP*, vol. 6, no. 1, p. 3). This statement, which various professional journals have or will soon adopt as policy, requires that authors relying on quantitative data sets deposit the data set (following a suitable embargo) in a public archive, and that publications using the data set indicate the location. Scholars whose research results rely on non-quantitative data are exempted from the requirement (although they are urged to include enough information in their publications so as to permit replication of their work).

This exemption could conceivably affect the field of comparative politics quite widely, since a great deal of work in the field (perhaps even most work, although it's difficult to know) uses qualitative not quantitative data. Much of such data is gathered through field work, which is the standard and perhaps dominant method of data collection in the comparative field. I have argued elsewhere (*PS*, Sept. 1995) that those of us who gather non-quantitative data have just as much responsibility to archive our information as our quantitative colleagues. I do not believe that logistical problems in archiving non-quantitative data are insurmountable, although recording and storing such data require less obvious techniques. But, good field work practice necessitates that we record carefully and thoroughly, and with computer technology now portable and inexpensive, there is little difference between recording and the ability to archive.

The main exception to the requirement to archive data gathered through field work comes from confidentiality concerns. There are research projects, especially those that involve elite interviewing, for which it would not be appropriate to make the data publicly available. I suspect, however, that insurmountable confidentiality concerns probably affect only a minority of us in the comparative field. In many instances, data can be rendered anonymous (e.g. migrant no. 55, city name, interview date), thereby protecting respondents. In other cases, confidentially is simply not an issue. In any event, the benefits of adhering to a replication standard are so enormous that we have an obligation, I believe, to subscribe to it in spirit, even if our obligations to our respondents force us to breach it in the letter.

The main benefit of a replication standard is not that others will in fact replicate our work, a tedious and often fruitless activity (although highly suitable for graduate students, who should be forced to perform replication experiments in their coursework). Instead, the main benefit is that adherence to this standard would improve our *own* research techniques in the field. If we conceptualize our research projects from the outset as potentially replicable, we will design our research more carefully, select respondents more systematically, and record interviews more fully.

In my article in *PS*, I proposed that articles or books relying on data gathered through field work contain a first footnote with the following: the dates and location of the field research; the number of persons interviewed; the selection criteria used for respondents; the types of questions asked; the average length of interview time; the source of funding; and any other information that would permit duplication of the field research process by other scholars. In addition, the footnote should indicate, along the lines proposed by King, when and where the data set will be archived, or if archiving is precluded because of confidentially concerns. I would like to see the journals in our field formally adopt the King replicability standard, with suitable alterations for research relying on field work, and suitable exceptions for compelling reasons of confidentiality.

Because so much of the comparative field uses field work, any replicability requirement carries with it issues specific to our field. There are, I am sure, practitioners who believe that field work is not and cannot be replicable, and that the kind of requirements I propose would impose artificial and unrealistic expectations on the process. I hope that the full gamut of views in the field are fully expressed and the issue debated thoroughly enough to guide journal editors in making the relevant policy decisions.

REBUTTALS AND COMMENTS

The APSA Section recognizes that this is a controversial topic which warrants debate, and intends to devote a portion of the next *Newsletter* to it. If you would like to contribute to this debate by rebutting or furthering the comments expressed above and in previous issues, please send your comments to the Editor Elect,

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Introducing the *European Journal of Industrial Relations*

Richard Hyman, Editor
University of Warwick

The first issue of the *European Journal of Industrial Relations* appeared in March 1995 and is published four-monthly thereafter. The Editor is Richard Hyman, Industrial Relations Research Unit, University of Warwick and the publisher is Sage. Our objectives are as follows.

This is a *European* Journal in the sense that its central focus is on industrial relations in Europe. This is not to imply that our intellectual vision is constrained by the idea of "Fortress Europe." Our continent is increasingly subject to global influences; and ideas and insights developed elsewhere in the world can be of relevance for the analysis of our own situation. Our priority is, however, to advance knowledge and understanding of industrial relations in that complex patchwork of nations which extends from the Atlantic to the Urals. In the case of eastern and central Europe — where industrial relations in the western sense is a recent and incomplete invention — serious academic analysis remains sparse; and even in western Europe there are many countries largely neglected in the industrial relations literature. This we hope to remedy.

At the same time, we believe that for a journal such as ours most single-country studies are of limited value. There is great need for more comparative research and analysis in industrial relations. Cross-national comparison forces us to relativize our perspectives on practices and institutions which are otherwise taken for granted, highlights issues (differences in similar countries, similarities in different countries) which require explanation, subjects our interpretations to a more rigorous test, and encourages more encompassing causal argument. Given the priority we attach to developing comparative work in the field, the initial issues of the *European Journal of Industrial Relations* will contain only papers which are cross-national in approach.

One reason for this emphasis is the importance we attribute to *theoretical* advance in a subject which, in its Anglo-Saxon homelands, has frequently been condemned for theoretical underdevelopment. All theory, it could be argued, is at least implicitly comparative; comparative analysis, almost inevitably, involves explicit theorizing. It is common to define theory as somehow opposed, on the one hand to detailed empirical research, on the other to the real world of practical people. We disagree. Empirical research has no more intellectual point than stamp-collecting unless it possesses theoretical relevance. Conversely, theorizing is sterile unless it connects with empirical evidence. Thus the *European Journal of Industrial Relations* will encourage work which is both theoretically informed (and, we hope, innovative) and empirically grounded.

The link between theory and practice is no less important. There has of late been much Philistine pressure for academic work to establish its "relevance." This is objectionable, not only because the time-horizons of scholars and practitioners are often very different, but also because the criteria of relevance of different actors (e.g. employers, trade unionists, government officials) are far from identical. Nevertheless, policy and practice in the real world of industrial relations are often based on inadequate information and incomplete understanding. We encourage our authors to make explicit the practical significance of their analyses, and to present their ideas and evidence in a form which is accessible to more than a select audience of academic specialists. Likewise, we welcome reflective contributions from practitioners.

We adopt a broad understanding of industrial relations. Traditionally, the subject has often been defined as a study of *institutions*: trade unions, employers' organizations, collective agreements, labour legislation. These are indeed essential components of our field. However, it is increasingly obvious that such institutions do not constitute a self-contained "industrial relations system" whose functioning can be studied in isolation from other societal dynamics. In the *European Journal of Industrial Relations* we will be concerned with developments both "above" and "below" the level of institutions: "above," by considering the impact on industrial relations of national and transnational political and economic transformations; "below," by sensitivity to the effects of detailed changes in work, employment and social identities.

Industrial relations, in the sense of the collective regulation of employment relations, became most strongly institutionalized in the mass industries of 19th-century capitalism: coal mining, railways, docks, large-scale manufacturing. For the most part these were *milieu* of tough and even brutalizing work, in which male manual employment predominated. The icons and ethos of traditional industrial relations — even in sectors of employment with very different characteristics — were typically shaped by these origins. In recent years, the study of work and employment has been enriched by a different repertoire of images, ideas and interpretations — most notably as a result of feminist scholarship. The "gendering" of work and employment, and also the impact of ethnicity and other components of social

Continued on Page 16

Luebbert Award Announcements

Frances Rosenbluth, the chair of this year's Luebbert Awards committee (and on behalf of the other committee members, Catherine Boone [University of Texas, Austin] and Kaare Strom [University of California, San Diego]), announced the winners of the Luebbert Awards which will be presented at the APSA national convention in Chicago.

The Luebbert book award goes to Crawford Young for *The African Colonial State in Comparative Perspective* (Yale, 1994). Young's book is breathtaking in its historical, geographical, and theoretical sweep. In comparing colonialism in Africa with numerous cases elsewhere, Young argues compellingly that the particular character of colonialism in Africa goes far in explaining why the legacy of imperialism was so much more debilitating than it was in other postcolonial societies. The lateness of colonial conquest in Africa made for critical differences; modern colonial conquest in Africa made for critical differences; modern doctrines of hegemony and sovereignty made for "deeper" and more thorough domination; modern technologies of warfare made conquest more violent and complete; and the voracious appetite of the twentieth-century colonial state for revenues made economic exploitation in sub-Saharan Africa more brutal and intense. The independent African state, Young shows, bears the heavy imprint of these attributes of its predecessor. By redefining the African state in terms of its colonial ancestry, he reframes the entire issue of postcolonial state failure in sub-Saharan Africa. *The African Colonial State* is an impressive achievement that will certainly be read as the basic work on the topic of comparative colonialism for a long time to come.

The Luebbert award for the best article in comparative politics for 1994 goes to David Laitin for "The Tower of Babel as a Coordination Game: Political Linguistics in Ghana," *APSR* 88:622-34. In searching for the article most worthy of this prize, one finds many fine pieces of scholarship that bring theory usefully to bear on important empirical issues in comparative politics. What distinguishes Laitin's article is its careful and creative application of game theoretic logic to an area of social policy such as language choice. That few have attempted to marry the rigor of game theory with the emotion-laden issues surrounding cultural and political identity is understandable, given the obvious perils inherent in applying simple models to such situations. Laitin, however, succeeds splendidly, moving deftly between theory and data. Because of his evident command of the empirical case, he manages to use the theory in a context-sensitive way to illuminate a little-studied, poorly understood, but important issue. Laitin's work, moreover, suggests that applicability of tipping models and related theoretical tools to a whole range of public policy issues that have not been analyzed rigorously. This is an instructive piece for how to conduct social science inquiry and an elegant example of how to communicate a complex problem.

1996 APSA Program

Karen Remmer (University of New Mexico) is the Comparative Politics Organized Section's APSA Program Chair for 1996. Please contact her with ideas and topics for papers, discussion panels and other interesting events for the APSA national convention. Karen Remmer can be contacted by e-mail at: remmer@bootes.unm.edu

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identities, are increasingly recognized as essential elements in understanding industrial relations. The *European Journal of Industrial Relations* will encourage assessments and definitions of our subject which embrace wider sensibilities and priorities than have prevailed in the past.

It follows that the Journal will welcome innovation, debate and controversy. We trust that most authors will dare to be adventurous; and that they will provoke responses from our readers. The Editor may incline to a strong and distinctive "line," but the *European Journal of Industrial Relations* is an open forum.

A key objective is to build intellectual bridges. Studies of industrial relations in Europe have in the past been fragmented by discipline, language and distinctive national traditions and institutions of academic production. Compartmentalization of academic activity leads inevitably to duplication of effort and circumscription of outcomes. Thus our aim is to assist the development of a community of industrial relations scholars which is both interdisciplinary and *international*. The value of investigation and analysis can often be greatly enhanced simply through greater awareness of practice and research in other disciplines and in other countries. Apart from the substantive articles published in this journal, a major contribution to cross-national dissemination of ideas and information will be generated by regular review articles covering recent work in industrial relations — in whatever discipline — in the various European countries.

This journal is launched with ambitious objectives. We aim to make a major contribution to teaching, research and policy in our field in a context of cross-national integration and of increasing interest in different national "models" of employment regulation. We trust that our readers will help shape the future of the *European Journal of Industrial Relations* and of industrial relations in Europe.

Those considering submitting a manuscript are advised to contact the Editor: fax +44 1203 524656; e-mail <irobrh@wbs.warwick.ac.uk>.
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